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ESSAYS

AND

OBSERVATIONS,

PHYSICAL AND LITERARY.

*Read before a Society in EDINBURGH,
and published by them.*

VOLUME II.



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ESSAYS

ON THE

OBSERVATIONS

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ESSAYS

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AND

OBSERVATIONS

PHYSICAL AND LITERARY.

ARTICLE I.

The Description of a new Plant ; by Dr. ALEXANDER GARDEN, Physician at Charleston in South Carolina.

DOCTOR GARDEN writes Doctor *Whytt*, that, in Summer 1754, he met, about a mile from the town of *New York* in *New England*, with a plant ; which, at first, he took to be a *hypericum*, but, on examining it, found it different : upon which he took down its characters, and sent them, some days after, to Miss *Jenny Colden* (daughter to the Honourable *Caldwallader Colden*), a very ingenious young Lady and curious Botanist. In return to this, Miss *Colden* sent Dr. *Garden* the characters of a plant which proves to be

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the same : it is N^o 153. of her collection ; and was first found by her, Summer 1753. Using the privilege of a first discoverer, she was pleased to call this new plant *Gardenia*, in compliment to Dr. *Garden*.

The Description of the GARDENIA, sent by Dr. Garden to Miss Colden.

ANONYMOS.

POLYADELPHIA ENNEANDRIA.

RADIX perpendicularis, in descensu fibras aliquot at plures cirros emittens, sensim attenuatur, simplex, mollis.

CAULIS simplex, fruticosus, teres, rectus, ex alis foliorum internodiis sesquipollicaribus laterales emittens ramulos, lineam crassus, cubitum plus minus altus, cavus, annuus, glaber.

FOLIA simplicia, sessilia, patula, bina ex opposito alternata, integerrima, obtusa, ovata, cordata in duo auricula, circa caulem supra se invicem expansa, ad basin extenduntur, nitida, supernè viridia, infernè glauca, nervo medio infra

frà prominulo, punctis lucidis *hyperici* foliorum instar perforata——dimidium pollicis plus minus latitudine, sesquipollicari fere longitudine.

PEDUNC. nunc alaris, nunc terminatrix, filiformis, femiuncialis.

CALYX perianthium pentaphyllum persistens, foliolis linearibus, lanceolatis, acutis.

COROLLA pentapetala pallidè rubra, bracteis lanceolatis, cum calyce situm alternantibus et longioribus.

STAMINA. *Filamenta* novem, leviter purpurea filiformia, in tria corpora ad basin coalita. Fasciculi isti filamentorum, interpositione trium corporum *nectareis* similium, à se invicem separantur. Haecce corpora, colore sunt bruneo-luteo, obtusa, crassa, breviora, receptaculo sessilia, hinc modice cava, inde gibbosa. *Antherae* subrotundae, parvae, luteae.

PISTILLUM germen trigonum ; styli tres, parum reflexi, teretes ; stigma nullum, vel saltem nudis oculis imperceptibile.

RECEPTAC. parvum cyathiforme excavatum.

PERICARP. capsula oblonga, acuminata, obtusè triquetra, trilocularis, trivalvis, tribus

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tribus fulcis per longitudinem decurrentibus.

SEMINA plurima, parva, obtusè cylindrica, receptaculo proprio per funem umbilicalem brevissimum in duobus ordinibus adhaerentia.

1. *Hypericum* inter *polyadelphia* *polyandria* collocat celeberrimus LINNÆUS; et optimo quidem jure: nam, in quacunque re variant inter se diversae hujus species, constanter tamen existunt filamenta numerosa; at in hacce planta semper novem inveniuntur filamenta, et semper in tria corpora vel fasciculos concresecunt.

2. In *hyperico* nunquam occurrunt *nectarea*; at in *anonymo* semper existunt tria, conspicua, et stamina ab invicem separantia.

3. Germen et pericarpium figurâ subrotundâ donantur; at in *anonymo* uterque triquetra forma gaudet.

4. In *hyperico*, calyx perianth. quinquepartitum; at in *anonymo*, perianth. pentaphyllum.

Miss

Miss COLDEN's Description of the same Plant.

N^o 153. GARDENIA.

COVER of the flower is a cup, composed of five lancet-shap'd leaves continuing.

FLOWER is five oval-shap'd leaves, longer than the cup, and spread out.

CHIVES are nine, placed in three bundles; every three are joined in one body for near half their length; their upper parts are fine threads; they are a little shorter than the flower-leaves. The *caps* are roundish.

There are three small oval-shap'd bodies, of a bright red colour, placed on the *seat* of the flower alternately with the bundles of *chives*.

PISTILL. The *seed-bud* (*germen*) is of a long ovally shape, with three deep furrows. The *stiles* are three threads of the length of the chives. The *tips* (*stigma*) are plain.

COVER of the seed is a long oval-shap'd box, of a dark red colour, with three deep furrows along it, and opening at three

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three parts at top. Before the seeds are ripe, it contains three cells ; but when the seeds are ripe, the divisions separate at the axis, and it contains but one cell.

SEAT of the seed is three ridges (being the inside of the three furrows of the said box) each with two rows of seeds.

SEEDS are numerous, small, and oval-shap'd.

ROOT is fibrous ; but, tho' fibrous, the most of them are about as thick as the stalk ; they are white and branched ; some of them are like fine threads.

STALK grows single, is round and smooth, and is branched out oppositely from the arm-pits of the leaves.

LEAVES stand thinly in pairs oppositely on the stalk and branches, they have no leaf-stalks, are oval-shap'd, about half as long as they are broad, broadest in the middle, are smooth, have a rib along the middle, with small transparent veins extending from it towards the edges, and the edges are intire.

The flowers are of a pale red colour ; they stand in clusters on the top of the stalk
and

and *branches*, and sometimes in pairs at the arm-pits of the *leaves*.

This plant grows in wet ground, and flowers in *August*.

OBSERVATION. The three chives *only* in each bundle, and the three oval-shap'd bodies on the seat of the flower, together with the seat to which the seeds adhere, distinguish this plant from the *hypericums*; and, I think, not only make it a different *genus*, but likewise makes an order which LINNÆUS has not.

ART.

ART. II.

*A Description of the Matrix or Ovary of the Buccinum Ampullatum**; by ROBERT WHYTT M. D. F. R. S. Fellow of the Royal College of Physicians, and Professor of Medicine in the University of Edinburgh.

NATURALISTS inform us that the *buccina* and *purpuræ*, the former towards the close of the Winter and the latter in the Spring, throw out a viscid glutinous humor; which thickening and becoming dry, forms a congeries of cells or receptacles connected together, something resembling the cods of white vetches, and containing the nascent *buccina* or *purpuræ*. This congeries of cells or pods is often found upon the shore, and sometimes mistaken for the hardened froth of the sea.

BUT whatever may be the manner in which these cells are formed, the *matrix*,
ovary

* The *buccinum ampullatum* is that in which the hermit-crab is found.

ovary or receptacle, in which the nascent *buccina ampullata* are found, has something remarkably curious in its structure.

TAB. i. *fig. 1.* represents the *matrix*, as it is called, of the *buccinum ampullatum*, sent me from South-Carolina by Dr. Garden; the length of which was full two foot.

a a a a a, &c. are the cells, receptacles or pods, composing this *matrix*, whose number amounted to 94. These pods, towards the extremity D, turned smaller.

ABCD, a band or ligament connecting the several cells or pods, of much the same kind of substance with the sides of the cells, but thicker and tougher.

THE pods or cells are composed of a thin, tough, transparent skin or membrane, and contain in their cavities a considerable number of small *buccina*.

Fig. 2. represents one of these cells intire, thro' the transparent membrane of which may be seen the *buccina* it contains.

Fig. 3. shews one of these cells or pods opened, that its *buccina* may be distinctly seen.

Fig. 4. exhibits several different views of the small *buccina*, all of their natural size.

IN some of the pods or cells, I found 32, in others, only 28 *buccina*: but, as towards the small end of the *matrix* the pods were less, and had fewer *buccina* in them, we may suppose that each pod might contain 25 *buccina*; in which case, the number in the whole *matrix* must have been 2350.

ART.

Fig. 1.st

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Tab: 1.st



Fig. 2.^d



Fig. 3.^d

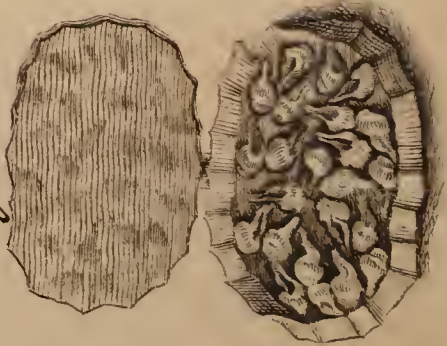


Fig. 4.th





ART. III.

Drawings of some very large Bones ; by
 GEORGE CLERK Esq ;

TAB. II. *fig. 1.* represents the *scapula* or shoulder-blade of an animal, found in a shell marl-pit near the town of *Drumfries*, of a monstrous size, measuring in length twenty one inches and eight tenths.

Fig. 2. is a rib twenty three inches long, and two and two tenths broad.

Fig. 3. and *4.* are also pieces of broken ribs, found in the same place ; where the whole or greatest part of the bones of the animal were afterwards discovered, but lost or destroyed by the country-people who found them.

As the bones described are at least one third larger than those of our biggest oxen or horses, they are supposed to be the remains of an *Elk* or some other animal not at present an inhabitant of this country.

THEY are just now in the possession of *George Clerk*.

ART.

ART. IV.

Observations on Light and Colours; by
 THOMAS MELVILL M. A. *

SECT. I.

On the Mutual Penetration of Light.

I. **O**NE of the first and greatest difficulties that occurs in reflecting on this subject, is, to conceive how it is possible that light

* Read *January 3. and February 7. 1752.*

Had the ingenious Author of this paper (who died *December 1753*, at the age of 27) lived to put the finishing hand to it, he would, probably, have added many things, and perhaps retrenched some others, by which it would have been rendered still more deserving of the approbation of the public. Mr. *Melvill* used to observe, that as, of all Sir *Isaac Newton's* discoveries, those relating to light and colours were perhaps the most curious; it was somewhat remarkable, that few, if any, of his followers had gone one step beyond him on these subjects, or attempted to compleat what *he* had left unfinished. Our Author, therefore, proposed to have applied himself particularly to the further illustration of the theory of light and colours. The following essay is a specimen of what might have been expected from him, and sufficiently shews the uncommon genius of its Author.

Fig. 1.st



Fig. 2.^d



Fig. 3.^d



Fig. 4.th





light can move thro' light in all imaginable directions, without occasioning the least perceivable confusion or deviation from its rectilinear course. Many have been induced, from this consideration, to believe it incorporeal ; and all who have thoroughly weighed the difficulty, have seen the necessity of ascribing a subtilty to it incomparably greater than we are led, by any *phænomena*, to ascribe to any other species of bodies in Nature. There is no physical point in the visible horizon which does not send rays to every other point ; no star in the heaven which does not send rays to every other star : the whole horizon is filled with a sphere of rays from every point in it ; and the whole visible universe, with a sphere of rays from every star. In short, for any thing we know, there are rays of light joining every two physical points in the universe, and that in contrary directions ; except where opaque bodies intervene.

2. THOSE who suppose that light is nothing else than vibrations or pulses propagated thro' a subtile elastic *medium* from the visible object to the eye, may perhaps remove the difficulty by ascribing a sufficient minuteness

ness to the particles of that *medium*; since we see, by experience, that sound in the air, and waves in the water, are conveyed in different directions, without sensibly interfering: but, as that hypothesis seems insupportable on other accounts *, we must endeavour to accommodate our solution to the only other conception we can frame of it; namely, that of particles actually projected from the luminous body.

3. IT is manifest, that, tho' the mere subtilty of the particles of light may tend to account for its easy passage, in all directions, thro' dense transparent bodies, it will not serve to explain its easy passage thro' other light equally subtile: but, for this purpose, it seems necessary to suppose light incomparably rare when at the densest; that is, that the semi-diameters of two of the nearest particles in the same or in different rays, soon after their emission, are incomparably less than their distance.

4. LET us consider a little the course of a particle of light from any of the remoter fixed

* *Newtoni Principia*, book 2. prop. 41. and 42. See also *Newton's Optics*, query 28.

ed stars to the human eye ; for instance, from the small one called the *Rider* in the tail of the *Great Bear* : The particles by which we see that star, have, in the first place, passed thro' the space surrounding it, in which there are probably several planets revolving, and which must be therefore so filled with a sphere of rays from each of them that they may be visible to an eye any where situated in those spaces ; after that, they have passed laterally thro' the whole torrent of light flowing from the star of the second magnitude which we see beside it ; and lastly, they have passed likeways across the whole ocean of the solar light, and all that light with which the space surrounding the sun is filled from all the comets, planets, and satellites ; and besides, in every physical point of their numerous journey from the *Rider* to our eye, they have passed thro' rays of light flowing in all directions from every fixed star in the visible universe : and yet, during the whole, they have never jostled against one particle of light ; otherways they could not have arrived in their true direction to our eye. This reflection cannot fail to suggest a general notion of the rarity and tenuity

nuity of light, far surpassing all the suppositions which are usually made about it.

5. THE chance which any one body has to jostle with others of like magnitude, is lessened in proportion to the bulk of the bodies with respect to the space in which they move. It must be therefore supposed, as we mentioned above, that the distance of the nearest particles, flowing in the same and in different lines, must exceed their diameter, not indeed infinitely, but a number of times utterly incomparable with all our ordinary numbers, in order that a particle may escape in one physical point of its progress: but, that it may pass freely on thro' the whole distance of the remotest fixed stars, it is evident, that this proportion of excess must be multiplied by a number again incomparable. But this excess, so increased, must be raised to a power whose exponent is a number equal to the number of all the fixed stars, planets, and comets. And lastly, if there is an elastic *medium* diffused thro' the mundane space, as the propagation of heat* and many other *phænomena* seem to insinuate; this last number must be at least doubled, if we would

* Newt. Opt. queries. *ad finē*

would express the proportion in which the distance of the nearest rays exceed the diameters of their particles: and yet this distance of the nearest rays, flowing from the same center, is so incomparably below our smallest measures, that there is no possibility of defining it.

6. HAD *Euler* considered this extreme rarity, as well as tenuity of light, which must be acknowledged by all who suppose that its particles are actually projected from the lucid body, he would not have alledged, that this opinion is inconsistent with the freedom and perpetuity of the celestial motions*.

7. SOME have thought, that, if the particles of light repel one another, their mutual perturbation may be prevented: but the contrary is manifest upon the least reflection; for tho', by that means, the particles might be prevented from striking, they must instantly turn one another from their rectilinear courses, as soon as they come, in different directions, within the reach of their mutual powers. Thus, we find by experience, it is impossible to make one stream of air penetrate another without confusion; for the two streams either unite into a common one with

* See his *Nova theoria lucis et colorum*.

an intermediate direction, or produce irregular eddies.

8. HERE, by the bye, we may see that the ingenious system of *Boscovich*, the *Roman* Professor, concerning the elements of matter *, whatever may be said for it from other considerations, gives us no assistance in comprehending the mutual penetration of light; for indivisible points, endued with an insuperable repulsive power, reaching to a finite distance, are as subject to interfere, as solid particles of a finite magnitude.

S E C T. II.

On the Heating of Bodies by Light.

9. IT appears, by Sir *Isaac Newton*'s experiments on the inflexion of light, that bodies act upon it at some distance; and that the same power, variously exercised in various circumstances, is the cause, likewise, of refraction and reflexion. We know no instance of any kind of attraction or repulsion in Nature which is not mutual; we observe likewise that bodies are heated by the influence of the sun's rays: it is therefore natural

* See his *Dissert. de lumine et de viribus vivis.*

tural to look upon this as the effect of the reaction of light upon bodies, and that, at a distance from them ; for, there is no reason to think that light produces heat by actually striking the solid parts of bodies, after we are satisfied that bodies produce the reflexion and refraction of light, without suffering it to come into contact with them.

10. FROM these principles it follows, that light, in passing out of one *medium* into another of different density, must always produce some degree of heat ; because it is partly refracted and reflected at the common surface : secondly, that, in passing forwards thro' the same homogeneous or perfectly transparent *medium*, it can produce no heat ; because there is no reflexion or refraction, no influence of the body upon the light, but every ray pursues its own right-lined course, as if it moved in a perfect void *.

11. HENCE it appears, that, in water, glass, and other transparent *mediums*, which
are

* Sir Isaac Newton, in the third book of his *Principia*, where he disputes concerning the tails of comets, lays it down as an obvious principle, *Quod radii solis non agitant media quæ permanant, nisi in reflexione et refractione.*

are warmed by the sun's rays, the heat must be propagated from their surfaces towards their central parts*.

12. HENCE likeways we understand why opaque bodies are sooner heated by the sun-beams than transparent ones; since, there are innumerable reflexions and refractions within their substances, besides what happen in common with transparent bodies at their superficial parts. As each colorific particle of an opaque body, by the reaction of the particles of light, must be somewhat moved when the light is reflected backward and forward between the same particles, it is manifest that they likeways must be driven backward and forward with a vibratory motion; and the time of a vibration will be equal to that which light takes in moving thro' a particle, or from one particle of a body to another adjoining. This distance in most solid opaque bodies cannot be supposed greater than $\frac{1}{12500}$ th of an inch, which
space

* I have found, by repeated trials, that the heat of water in deep lakes decreases regularly from the surface downwards.

space a particle of light describes in $\frac{1}{125,000,000,000,000}$ th of a second. With so rapid a motion therefore may the internal part of bodies be agitated by the influence of light, as to perform 125,000,000,000,000 vibrations or more in a second of time! The arrival of different particles of light at the surface of the same colorific particle in the same or different rays, may disturb the regularity of their vibrations, but will evidently increase their frequency, or raise still minuter vibrations among the parts which compose these particles; by which means the intestine motion becomes more subtile and thoroughly diffused. If the quantity of light admitted into the body be increased, the vibrations of the particles must likewise increase in magnitude and velocity; till, at last, they may be so violent as to make all the component particles dash one another to pieces by their mutual collisions: in which case, the colour and texture of the body must be destroyed. Thus may we form, from known principles, some imperfect conception of the manner in which bodies are heated and burned by the action of light: More than an imperfect notion of these se-

cret

cret operations of Nature is not to be expected; for they certainly depend, in great measure, upon laws and principles utterly unknown to us.

13. IF one beam or ray of light, by passing straight onwards thro' the same pellucid substance, can communicate no heat to its internal parts; neither will the greatest quantity of rays, tho' crowded into the narrowest space, by crossing one another. From hence it follows, that the portion of air which lies in the *focus* of the most potent *speculum* is not at all affected by the passage of light thro' it, but continues of the same temperature with the ambient air; altho' any opaque body, or even any transparent body denser than air, when put in the same place, would be intensely heated in an instant.

14. THIS consequence, evidently flowing from the plainest and most certain principles, seems not to have been rightly understood by many philosophers*: for which reason,

I

* See *Boerhaave* element. chem. tom. 1. on fire, coroll. 5. after exper. 14. and coroll. 1. and 7. after exper. 17. See also *Rutherford's* system of natural philosophy, prop.

I thought it might be worth while to say something in explication of it. The easiest way to be satisfied of the matter experimentally, is, to hold a hair or down immediately above the *focus* of a *lens* or *speculum*, or, to blow a stream of smoke from a pipe horizontally over it; for, if the air in the *focus* were hotter than the surrounding fluid, it would continually ascend upon account of its rarefaction, and thereby sensibly agitate these slender bodies. Or a *lens* may be so placed as to form its *focus* within a body of water or some other transparent substance, the heat of which can be examined from time to time with a thermometer: but care must be taken in this experiment to hold the *lens* as near as possible to the transparent body; lest the rays, by falling closer than ordinary on its surface, should warm it more than the common sun-beams.

15. IT is well known that the rays of light, by passing obliquely thro' our atmosphere,
are

366. of the astronomical part; and *Nolet leçons de physique*, tom. 4. The silence of most physical writers, concerning this paradoxical truth, makes it probable that they were unacquainted with it.

are inflected into a curve by the continued infraction arising from the continual increase of its density; therefore they must produce some degree of heat in every part of their progress thro' it [N^o 10.]: But, as the whole successive refraction is just equal to the single refraction that would be made in passing at once from the celestial spaces into a *medium* as dense as the lowest part of our atmosphere *, and all the successive reflexions that can be made from every different *stratum*, are but equal to what would be made at once from the surface of a *medium* of the same density; it easily appears, by comparing the densities of air and water, and their respective signs of refraction, that all the refraction and reflexion which the whole depth of our atmosphere produces is much less than what happens at one surface of water; and consequently, the heat produced in our atmosphere, by the immediate action of light upon it, must likewise be much less than what is raised in water. The air seems to have the greatest part of its heat communicated to it from the opaque vapours which

* *Newt.* Opt. book 2. part 2. prop. 10.

which float in it and the general surface of sea and land to which it is contiguous.

S E C T. III.

On the silver-like Appearance of Drops of Water on the Leaves of Colewort.

16. IT is common to admire the volubility and lustre of drops of rain that lie on the leaves of *colewort* and some other vegetables ; but no philosopher, as far as I know, has put himself to the trouble of explaining this curious *phænomenon*. Upon inspecting them narrowly, I find, that the lustre of the drop arises from a copious reflexion of light from the flattened part of its surface contiguous to the plant : I observe further, that, when the drop rolls along a part which has been wetted, it immediately loses all its lustre ; the green plant being then seen clearly thro' it : whereas, in the other case, it is hardly to be discerned.

17. FROM these two observations laid together, we may certainly conclude, That the drop does not really touch the plant when it has the mercurial appearance, but hangs

in the air at some distance from it, by the force of a repulsive power; for, there could not be any copious reflexion of white light from its under surface, unless there were a real interval between it and the surface of the plant*. [See TAB. iii. Fig. 2.]

18. IF

* *Newt.* Optics, query 29.

Let AB, TAB. iii. Fig. 4. represent the extremity of any repulsive body immersed in water, for instance a slice of *colewort* leaf, CL and DM, the convex surfaces of water immediately surrounding it, and CD perpendicular to AB, the common tangent of these curves, which will be the continuation of the general surface of the water. The forces with which any two particles, E and F, are pressed by the water in the directions EG, FH perpendicular to KB, are known to be as KG and KH, and the repulsive powers which balance them must be in the same proportion. If therefore the relation between the ordinate and absciss in the curve DM could be any way found by experiment, the law of the repulsive power might be determined, upon supposition that the particles are influenced by no force but the repulsive power of the line KB and the gravity of the superincumbent fluid; but their mutual attraction, which tends to lessen their lateral tendency, must be likewise taken into the account in order to an exact determination.

Before I leave this subject of attraction and repulsion, I beg leave to propose to the Society, the spontaneous motions of light bodies on the surfaces of fluids, as a thing worthy of being inquired into; for, tho' it be manifest in general that they depend upon the different figures of the surface, it is far from being an easy matter to explain the particular cases by mechanical or hydrostatical laws. The following account

of

18. If that surface were perfectly smooth, the under surface of the drop would be so likewise ; and would therefore show an image of the illuminating body by reflexion, like

of the *phænomena* may be useful towards such an enquiry.

Case 1. Suppose a fluid which is attracted to the side of its containing vessel, and consequently is elevated, at the sides, into a concave surface : If a body be immersed which attracts the fluid, and is therefore surrounded likewise with a concave elevation of the fluid ; as soon as the two elevations begin to join, the body will move towards the side of the vessel with an accelerated motion. *Case 2.* Suppose a fluid which is formed into a convex surface, either by the repulsive power of the containing vessel or cohesive force of its own particles : If a light body be immersed which attracts the fluid ; as soon as its surrounding elevation begins to join with the lateral depression of the fluid, it will begin to move towards the middle of the vessel ; and, if it be brought by force towards the side, it will recede from it again with an accelerated motion.

In both the first and second *cases*, if the attracting body be held fast, and the whole fluid made easily moveable with its containing vessel, it will remove to or from the attracting body in the same manner as the attracting body did with respect to it ; *i. e.* in the first case, the whole fluid will move so that the attracting body may come to its edge ; and in the second, so that it may recede from it. *Case 3.* If, in a fluid which is attracted by the sides of its vessel, a body be immersed which repels the fluid, and is therefore surrounded with a ditch or convex depression of the fluid ; as soon as that depression begins to join the elevation of the fluid at the sides, it will recede towards the middle ; and, if forcibly

like a piece of polished silver : but, as it is considerably rough and unequal, the under surface becomes rough likewise ; and so, by reflecting

forcibly brought to the side of the vessel, will fly from it with an accelerated motion. *Case 4.* If, in a fluid which is formed into a convex surface at the sides, a repelling body be immersed ; as soon as its depression begins to unite with the lateral depression of the fluid, it will move towards the side with an accelerated motion. In these two last cases, the same observation holds as with respect to the first and second, *viz.* that the whole fluid will move with correspondent motions by the force of reaction, if the repelling body be held fast. *Case 5.* If two bodies be immersed in a fluid, which each of them attracts ; as soon as their elevations begin to join, they will rush towards one another with equal forces and accelerated motions, and continue to adhere together. *Case 6.* If two bodies be immersed in a fluid which they repel ; as soon as the two depressions that surround them begin to interfere, they will likewise rush together with an accelerated motion. *Case 7.* If two bodies be immersed in a fluid, the one of which attracts and the other repels it ; as soon as the depression surrounding the one begins to join with the elevation of the other, they will mutually fly from each other. *Lastly,* If a body be immersed in a fluid which it attracts in one part and repels in another, it will approach to or recede from other bodies and parts of the fluid, differently according to its situation, by the rules above laid down.

The different figures ascribed to the surface of the water in these several cases are plainly discernible by the sight : if the experiments are made with candle-light, they are distinguished by the shadowy or luminous rings which they project

reflecting the light copiously in different directions, assumes the resplendent white colour of unpolished silver.

19. AFTER it is thus proved by an optical argument that the drop is really not in contact with the plant which supports it, we easily conceive whence its wonderful volubility

project on the bottom of the vessel, according as they are convex or concave.

Some writers have been so inattentive as to ascribe the motions in the first case to an immediate attraction between the swimming body and the side of the vessel. See *Helfham's Lectures*. Before I had observed the fourth and sixth cases, I thought the *phænomena* might be all explained from this principle, that the light bodies always tend to the highest parts of the water. It has been suggested to me by some, that this tendency, combined with the greater or lesser immersion of the bodies, upon account of the ring of water which they elevate or depress, may produce all the different cases: and by others, that the whole is explicable from the single principle of attraction between the parts of water which causes two drops to run into one. I believe it will be found, on due consideration, that none of these accounts is satisfactory; but there is no reason to despair of coming to the bottom of these *phænomena*; since other motions of a like kind have been successfully explained. Thus the running of a drop of oil towards the concourse of two glass-planes and the motion of a bubble on the surface of liquors, when the glass is held obliquely towards that point, where the glass is inclined to the liquor in the smallest angle, are easily understood from the direction of the compound force, with which the drop and bubble are acted.

bility arises, and why it leaves no tract of moisture where it rolls.

20. FROM the like reasoning, we may conclude, That, when a smooth needle is made to swim, it does not any where touch the water, but forms around it, by its repulsive power, a ditch or bed, whose concavity is much larger than the bulk of the needle. [See TAB. iii. Fig. 3.] And hence it is easy to understand how the needle swims upon a fluid lighter than itself; since the quantity of water, displaced by it, may be equal to the weight of the needle. *Phænomena* of this kind, instead of being reduced to hydrostatical principles, are commonly attributed to the mere tenacity of water, and even used for measuring its cohesive power. See *Musschenbroeck, Elementa Physices*.

21. THIS instance furnishes us with a just and necessary correction of the common hydrostatical law, That “the whole swimming
“body is equal in weight to a quantity of
“the fluid whose bulk is equal to that of
“the part immersed:” for, to comprehend this, as well as all ordinary cases, it should be said more generally, That “the whole
“weight of the swimming body is equal to
“the

“ the weight of the quantity of the fluid displaced by it.

22. THESE *Phænomena* appeared to me worthy of observation here: both because they shew the fertility of optical principles in leading to the knowledge of things otherwise inaccessible; and because they exhibit a clear specimen of a repulsive power, similar to that which we suppose necessary for the reflexion of light from the anterior surface of a denser *medium*. Nor do I see how it is possible to account for the suspension of the drop in the air by *comparative attractions*; into which some other appearances of repulsion have been, perhaps, not unsuccessfully, resolved*.

SECT. IV.

On the Change which coloured Bodies undergo in different Lights.

23. SIR *Isaac Newton* has abundantly proved, by a variety of arguments, that the ordinary colours of natural bodies arise solely from

* See *Jurin* on capillary attraction, at the end of *Cote's hydrostatical Lectures*.

from the compounded colour of those rays which they reflect ; their colour being denominated by the species of those primitive rays which they reflect in greatest plenty : but this part of the *Newtonian* doctrine will receive further confirmation by examining the different colours which the same body assumes when illuminated by different lights ; and which may be called, in distinction from the former, their extraordinary colours.

24. BODIES of all the principal colours, *viz.* red, yellow, green and blue, are very little altered when seen by the light of burning spirits : but, if salts be continually mixed with them during the burning, different changes ensue.

25. WHEN sal ammon. potash or alum are infused, the colour of red bodies appears somewhat faded and dirty : green and blue appear much the same as in candle-light ; both being faint and hardly distinguishable : white and yellow are scarcely at all affected.

26. WHEN nitre or sea-salt are plentifully mixed with the burning spirits, and the whole is stirred about briskly ; the brightest red bodies, seen by the light then emitted, are reduced to a dirty tawny brown, that
seems

seems to have nothing of redness in it: green is transformed into another sort of brown, only distinguishable from the former by a certain inclination to a livid olive-colour; when nitre is mixed with the spirits, one may still see some remains of a greenish colour, unless it be poured *in* very plentifully: dark blue is hardly to be known from black, except that it appears the deeper black of the two: light blue is changed into a very light brown of a peculiar kind: white assumes a livid yellowish cast: and yellow alone appears unaltered and extremely luminous. These experiments I made with different sorts of rich-coloured bodies, as silks, cloths and paints. Polished copper, which has contracted from the air a high-flaming colour, is reduced by the same light into the appearance of yellow brass; the faces and hands of spectators appear like those of a dead corpse; and other mixed colours, which have red or green in their composition, undergo like changes.

27. HAVING placed a paste-board with a circular hole in it between my eye and the flame of the spirits, in order to diminish and circumscribe my object, I examined the

constitution of these different lights with a prism, (holding the refracting angle upwards) and found, that, in the first case [N^o 25.], when sal ammon. alum or potash fell into the spirits, all sorts of rays were emitted, but not in equal quantities; the yellow being vastly more copious than all the rest put together, and red more faint than the green and blue.

28. In the light of spirits mixed with nitre or sea-salt, I could still observe some blue, tho' excessively weak and diluted: with the latter, the green was equally faint; but, with the former, pretty copious. But, when either of these salts were used, I could hardly see any vestige of the red at all, at least when they were poured *in* plentifully, and the spirits constantly agitated. At every little intermission indeed the red rays would show themselves very manifestly below the hole, and red bodies seen by that light resumed somewhat of their ordinary colour: and it was very entertaining to observe how both would vanish again at once, as soon as the salting and stirring were renewed.

THE proportion in which the bright yellow exceeds the other colours in this light,
is

is still more extraordinary than in the former: infomuch that the hole feen thro' the prism appears uniformly of this yellow, and as diftinctly terminated as thro' a plain glafs; except that there is adjoining to it on the upper fide a very faint fteam of green and blue. White bodies illuminated with it, appear alfo thro' the prism perfectly well defined; both which are very furprizing *phænomena* to thofe who have been accuftomed to the ufe of the prism in other heterogeneous lights, where it never fails to throw confufion on the extremities of all vifible objects.

29. BECAUSE the hole appears thro' the prism quite circular and uniform in colour; the bright yellow which prevails fo much over the other colours, muft be of one determined degree of refrangibility; and the tranfition from it to the fainter colour adjoining, not gradual, but immediate.

30. UPON examining soap-water-films in the fame light, I could only obferve luminous bands feparated by dark ones; the green and blue being too weak to affect my eye in this view. It would be needlefs labour to enter here into a particular detail of the reafons of the different transformations of coloured

loured bodies, above related [N° 24, 25, and 26.] ; since, in general, it is evident enough, that they are owing to the different compositions of the lights with which they were illuminated: the experiments with the prism [N° 27, 28.] are of themselves a sufficient commentary upon the rest.

S E C T. V.

A Remark on EULER's Nova Theoria Lucis et Colorum.

31. EULER, in that treatise, (published lately along with some other tracts, under the title of *Opuscula Mathematica*) endeavours to amend the *Huygenian* hypothesis of vibrations, and support it against the objections which made *Newton* and his followers reject it: we shall not enter here upon the discussion of that question ; as it would require a discourse of considerable length ; and the rather, that the *Newtonian* theory of light and colours depends not on any particular hypothesis with respect to the intimate nature of light (in like manner as his system of universal gravitation is independent of all hypotheses

potheses

potheses concerning the cause of gravity). In his Optics, he lays down his discoveries at full length, without ever inquiring whether light consists in vibrations propagated thro' a fluid or of particles projected in straight lines from the luminous body: and, in his queries, where he touches this matter *, he seems to be more positive in rejecting the hypothesis of vibrations, than in establishing any other.

32. BUT *Euler* likewise advances a new notion with respect to the origin of colours in opaque bodies, which is intirely inconsistent with the principal part of Sir *Isaac Newton's* doctrine. He supposes, that coloured bodies reflect the sun's incident white light from their anterior surface; but, that the particular species of light, by which they appear coloured, is properly emitted by the parts of the body: for instance, he imagines that vermilion does not appear red by a more copious reflexion of red than of other incident rays, but by the new emission of red rays from the particular velocity of vibration which its elastic parts are capable of conceiving by the impulse of the incident light.

33. IT

* *Newt.* Opt. quer. 28. and 29.

33. IT is a sufficient refutation of this system, that no *phænomena* prove or require its existence: whereas *Newton's* theory not only solves the *phænomena*, but is directly drawn from a multitude of experiments. According to *Euler's* hypothesis, a body of one colour, placed in homogeneous light of another, ought not to appear of the colour of the light, but of a middle one between that and its own natural colour; which is contrary to experience *.

34. If it should be said, That none of the incident light is capable of qualifying the body for emitting its proper colour, but rays of the same colour: that which he calls new light emitted will be, in his scheme, more properly incident light reflected.

35. THE chief or only fact which seems to have led him into that opinion, is, that there are many coloured bodies, such as metals, which are capable of receiving a fine polish; and therefore of reflecting regularly the images of other objects, and at the same time retain their proper colour by which they are seen in all positions. *That* light by which we see in them the images of other objects,
he

* *Newt. Opt.* book 1. part 2. prop. 10.

he acknowledges to be incident light properly reflected ; but the other, he supposes, is properly emitted from the colorific parts of the body. But what necessity is there of recurring to this supposition, when we know, previously, that the component parts * of all opaque bodies are transparent ; that, from every transparent body, there is a double reflexion ; part of the incident light being reflected at the first surface, and a part of what passes thro' the first, reflected at the second ? and when we know, further, that very thin bodies, (as soap-bubbles, *Muscovy*-glass, and air in a fracture of glass or ice, or between two *lenses*) while they reflect some rays of all colour from the first surface, reflect only particular colours at the second † ? Do not these facts lead us naturally to suppose the first sort of light to be only a part of the incident light reflected from the first surface of the body ; and the second, a part of what had passed on, reflected from the posterior surfaces of the superficial particles ?

S E C T.

* *Newt.* Opt. book. 2. part 2. prop. 2.

† *Ibid.* book 2. part 3. prop. 12.

S E C T. VI.

Concerning the Cause of the different Refrangibility of the Rays of Light.*

36. IN order to account for the different refrangibility of the differently-coloured rays, Sir *Isaac Newton* † and several of his followers have supposed, that their particles are of different magnitudes or densities: but, if there be any analogy between gravity and the refractive power, it will produce equal perpendicular velocities in all particles, whatever their magnitude or density be; and so all sorts of rays would be still equally refrangible.

37. IT seems therefore a more probable opinion, which others have advanced, that the differently-coloured rays are projected
with

* Altho' the doctrine contained in this section has been already published in the Philosophical Transactions for 1753; (vid. vol. xlviii. part 1. p. 262, &c.) having been communicated to the Royal Society, by the Author, in a letter to the Reverend Dr. *James Bradley* D. D. F. R. S.; yet it could not be omitted here, on account of its connexion with some of the queries that follow; besides that it contains several illustrations not to be found in the Transactions.

† *Newt.* Opt. query 29.

with different velocities from the luminous body: the red, with the greatest; violet, with the least; and the intermediate colours, with intermediate degrees of velocity: for, upon this hypothesis, it is manifest, that they will be differently refracted in the prismatic order; according to observation. Since, according to Sir *Isaac Newton's* doctrine of refraction now generally received, the velocity of a ray, after entering any new *medium*, is, to its former velocity, as the sine of incidence to the sine of refraction *; if all the colours move with equal swiftness in any one *medium*, their velocity will necessarily become unequal, upon entering a denser *medium*, in the inverse proportion of their several sines of refrangibility: tho' we suppose, therefore, the sun's rays to be emitted with one common velocity, it will follow that their velocities are unequal in air, glass, water, or any transparent body, whose refractive density differs from that of the solar atmosphere †. This consideration is sufficient to take off the appearance of improbability from our *hypothesis*.

* *Newtoni Principia*, lib. 1. prop. 95.

† See below, query 3.

38. ON supposition that the different refrangibility of the rays of light arises solely from their different velocities before incidence; these velocities must be, to one another, nearly as their sines of refraction.

39. SIR *Isaac* found their sines of refraction from glass into air, beginning from the extreme violet, to be * as 78, $77\frac{7}{9}$, $77\frac{2}{3}$, $77\frac{1}{2}$, $77\frac{1}{3}$, $77\frac{1}{5}$, $77\frac{1}{8}$, 77; the sine of incidence being 50: from whence their sines of refraction out of air into glass, beginning from the extreme red, and ending with the extreme violet, are found to be as † 78000, 77873, 77797, 77663, 77496, 77330, 77220, 77000; the sine of incidence being 120120. These numbers, therefore, nearly express the velocities in air, of the several rays, before their incidence ‡.

40. HENCE

* *Newt.* Opt. book 1. part 2. prop. 3.

† The extreme sines are plainly reciprocal to the former; and those of intermediate colours are fourth proportionals to the sine in Sir *Isaac*'s experiment, 77 and 78.

‡ The quantities which give the accurate proportion of the velocities, before incidence, must be in a constant *ratio*: the sines of refraction, by which the above calculations are made, have this condition: but, it is otherways manifest, that they give only a gross approximation to the truth.

From

40. HENCE their velocities in any other medium, may be found ; for, they are, to these,

From what follows, perhaps, an exacter computation might be made, if a proper mean angle of incidence were made use of, altho' the quantities in the canon are really not in a constant *ratio*.

TAB. iii. *fig. 1.* Let two rays, falling in the same line of incidence IC, with different velocities, upon AB the surface of a denser medium, be refracted into different lines CR, CV. Taking any line CD in the perpendicular to represent the total action of the refracting power on the less refrangible ray, and CE on the more refrangible : If, thro' D and E, parallels to IC be drawn, meeting the refracted rays in V, R and G ; it is plain, that CR, CV will be, as their respective velocities after refraction ; and DR, EV, as their velocities before incidence. Since the whole acceleration which a given power produces in a body, is, *cæteris paribus*, as the time in which it operates ; CD must be to CE nearly as the time which the swifter ray takes to pass thro' the refracting space, to that which the slower ray takes in passing thro' the same, inversely, as their velocities before incidence ; that is, as EV to DR : but CD is likewise to CE as DG to EV ; therefore DR, EV and DG, are continued proportionals ; therefore DR is to EV in the subduplicate ratio of DR to DG : but DR is to DG in a ratio compounded of DR to DC, and DC to DG, that is, in the compounded ratio of S, DCR to S, DRC and of S, DGC to S, DCG ; wherefore DR is to EV in the subduplicate ratio of S, DCR \times S, DGC to S, DCG \times S, DRC ; that is, “ The velocities before incidence are nearly in the direct “ subduplicate ratio of these sines and the reciprocal sub- “ duplicate ratio of the sines of the excesses of the common “ angle of incidence above the several angles of refraction.”

these, as the sine of incidence to the sine of refraction, when a ray passes from air into the given *medium* *.

41. WHILE the differently-coloured rays are supposed to move with one common velocity, any pulses, excited in the ethereal *medium*, must overtake them at equal distances; and therefore the intervals of reflexion and transmission, if they arise in this manner, as Sir *Isaac Newton* conjectures, would be all equal: but, if the red move swiftest, the violet slowest, and the intermediate colours with intermediate velocities; it is plain, that the same pulses must overtake the violet soonest, the other colours in their order, and, last of all, the red; that is, the intervals of the fits must be least in violet, and gradually greater in the prismatic order; according to observation.

42. As the proportion between these intervals in red and violet can be assigned by experiment, and the proportion of their velocities in any *medium* likewise, by N° 40.; the velocity of the ethereal pulses in any *medium*, and their distance from one another, may be thence computed by the following rule:

“ Multiply

* *Newt. Princip. lib. 2. prop. 9.*

“ Multiply the product under the velocities
 “ of the red and violet rays by the difference
 “ of the intervals of their fits; then divide
 “ by the difference of the two products
 “ which are formed by multiplying the in-
 “ terval of the fits in red by the velocity
 “ of the violet, and the interval of the fits
 “ in violet by the velocity of red :” the quo-
 tient shall express the velocity of the ethereal
 pulses *.

43. THE velocities of the red and violet
 in air, are, by the above estimation, as 78
 and 77 †; and the intervals of their fits are,
 by experiment ‡, as 100 and 63: from
 whence, by the canon now laid down, the
 velocity of the ethereal pulses is found to
 be, to that of red light, as 79763 to 78000. As

* Let C denote the celerity of the ethereal pulses, V the
 velocity of red light, and v that of violet, I and i the in-
 tervals of their fits, and D the perpendicular distance of two
 succeeding pulses: it is plain, from the nature of the hypo-
 thesis, that I is to D as V to $C - V$, and again, D to i as
 $C - v$ to v ; therefore, *ex æquo*, I is to i as $CV - Vv$ to
 $Cv - Vv$: from which arises the equation

$$C = \frac{I - i \times Vv}{I \times v - i \times V}.$$

† In the celestial *medium* they are less, [No 40.] but very
 nearly in the same proportion.

‡ *Newt. Opt.* book 2. p. 1. observat. 14.

As light moves from the sun to us, by Dr. *Bradley's* latest computation *, in 8', 12", the pulses of the ethereal fluid will be propagated thro' the same space in 8', 1".

44. THE distance between the ethereal pulses, is, to † the interval of the fits in red, as the difference between the velocity of the ethereal pulses and that of red light is to the velocity of red light ; that interval, therefore, is not much more than $\frac{1}{44}$ th of the interval of the fits in red, and therefore does not much exceed $\frac{1}{245793}$ of an inch ‡.

45. THE velocity of the ethereal pulses being determined, as above, from the intervals of the fits in the two extreme colours, as found by experiment, the intervals of the fits in the six intermediate rays may be calculated from theory; for the interval in any one colour must be, to that in red, as a product under the velocity of the given colour and the excess of the velocity of the ethereal pulses above that of red, is to a product under the velocity of red and the excess of the

* See *Eames* Abridg. tranfact. vol. vi. p. 157.

† See note * to foregoing page.

‡ See the table of the thickness of coloured plates in *Newt. Opt.* part 2. book 2.

the velocity of the ethereal pulses above that of the given colour: but, even upon the supposition of the truth of our theory, an exact coincidence between calculation and experiment is not to be expected till the velocities of the rays be more accurately determined.

46. UPON the hypothesis of the different velocities of different colours, we may understand, at least in general, whence it is, that the intervals of the fits may bear a proportion some way related to the spaces* occupied by the several colours in the spectrum; an analogy otherways very unaccountable! Since, from the velocities of the several rays upon which the intervals of the several fits depend, arise likeways their several degrees of refrangibility, which determine the space occupied by each in the spectrum.

AND thus likeways we may conceive, how the different rays are qualified to produce different sensations in the mind: for, having different degrees of impulsive force, they may cause vibrations of different magnitude or velocity in the optic nerve; by which, according

* Compare *Newt. Opt.* b. i. part 2. prop. 3. with b. ii. part 3. prop. 16th.

according to the laws of our constitution, the ideas of different colours may be excited*; in like manner as the ideas of different tones arise from different vibrations of the air communicated to the auditory-organ. It has been said, That the different sensations excited in the mind cannot arise from the different force of the particles of light; since the colour of homogeneal rays is not altered by passing thro' different *media*, tho' their velocity be thereby always increased or diminished †. But it ought to be considered, that every ray, as it must pass at last thro' the humours of the eye in order to vision, falls upon the *retina* with one given velocity, whatever number of refractions it has previously undergone: for the velocity of any ray in any one *medium* being, to its velocity in any other *medium*, in a constant proportion, *viz.* the inverse of the sines of incidence and refraction, when a ray passes from the one into the other; it is manifest, that each ray must have a certain determined velocity in any given *medium*, which cannot be either increased or diminished by making the ray pass

* *Newton's Optics*, query 13.

† *Musschenbroeck*, *Elementa Physices*, § 1161.

pass previously thro' any number of transparent bodies any how disposed*.

It is impossible therefore to know, whether an alteration of the swiftness, with which a homogeneal ray strikes the *retina*, would alter its colour; I mean the sensation of colour produced by it in the mind: since it is impossible to alter, at pleasure, the density of that fluid which determines its final velocity.

ONE may distinguish two different effects of the refractive power on the rays of light, *viz.* the change of direction and change of velocity. Sir *Isaac Newton* has proved with respect to the first, That it is different in the differently-coloured rays, and of a determined degree in each: he has further proved, That refraction, considered in its first effect, does not change the colour of any simple ray.

But it appears, from what we have now said, That none of his experiments prove the

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immutability

* Here it is proper to observe, that the *hypothesis* which supposes the intervals of the fits to be determined by the velocity of the ray, agrees well with a remarkable observation of Sir *Isaac Newton* (*Optics*, b. ii. part. 1. observ. 21.): *viz.* That these intervals in any *medium*, at a given angle of incidence, are of a given magnitude, without regard to the density of the surrounding *medium*.

immutability of simple rays by the second effects of refraction.

47. As it is of great consequence in philosophy to distinguish between facts and hypotheses, however plausible; it ought to be observed, that the various refrangibility, reflexivity, and inflexibility of the several colours, and their alternate dispositions at equal intervals to be reflected and transmitted, which are the whole ground-work of the *Newtonian* system, are to be considered as certain facts deduced from experiment: but whether the velocities of the different rays are exactly equal, or different in the manner now described, is no more than probable conjecture; and, tho' this point should be decided by a method proposed afterwards, it would still continue uncertain, whether the fits of reflexion and transmission are occasioned by an alternate acceleration and retardation of the motion of light, or in some other manner*.

And,

* For instance, it might be supposed, that every particle of light has two contrary poles, like a load-stone; the one of which is attracted by the parts of bodies, and the other repelled; and that, besides their uniform rectilineal motion, the particles of differently-coloured rays revolve in different periods round their center: for thus, their friendly and unfriendly

And, after all, it is no more than probable conjecture, that such an alternate acceleration and retardation is brought about by the influence of pulses excited in the ethereal *medium*: nay there are some circumstances in these *phænomena* that seem hardly intelligible by that hypothesis alone; as, why the intervals of the fits are less* in denser *mediums*; and why they increase so fast and in so intricate a proportion, according to the obliquity† of incidence.

48. ACCORDING to Dr. *Bradley's* beautiful theory of the aberration of light, the stars appear to be removed from their true places to a certain distance, by the proportion which the velocity of the earth bears to the velocity of light: It is plain therefore, that, on our hypothesis, a star must have a different apparent place for every different colour; that is, its apparent disk must be extended by the aberration into a longitudinal form resembling the prismatic spectrum, having
its

friendly poles being alternately turned towards the surfaces of bodies, they might be alternately disposed to reflexion and transmission; and that at different intervals, in proportion to the periods of their rotation.

* *Newt.* Optics, b. 2. part 3. prop. 17.

† Prop. 15. *ibidem*.

its red extremity nearest to its true place. In the stars situated near the pole of the ecliptic, its length should continue always the same, tho' directed along all the different secondaries of the ecliptic in the course of a year : but, in those which lye in or near the plane of the ecliptic, it should be greatest at the limits of the eastern and western aberrations ; the star recovering its colour and figure when the true and mean places coincide. But, there is no hope of discovering, whether our hypothesis be true or false, by this consequence of it ; for the greatest length of the dilated disk, being, to the whole aberration, as the difference of the velocity of red and violet to the mean velocity of light, *i. e.* as 1 to 77 nearly, (N^o 39.) cannot much exceed one fourth part of a second ; for the greatest aberration is but about twenty seconds.

49. THE time which the extreme violet takes to move thro' any space must be, to that which the red takes, as 78 to 77. If *Jupiter* be supposed in a quadrate aspect with the sun, in which case the eclipses of his satellites are most commodiously observed, his distance from the earth being nearly equal
to

to his distance from the sun ; light takes about forty one minutes of time in passing from him to the earth : therefore the last violet light which a satellite reflects, before its total immersion into the shadow of *Jupiter*, ought to continue to affect the eye for a 77th part of 41', or 32'', after the red reflected at the same time is gone : that is, A satellite, seen from the earth, ought to change its colour above half a minute before its total immersion from white to a livid greenish colour, thence into blue, and at last evanish in violet. I need scarcely observe, that the same *phænomenon* should take place in the time of emerfion, by a contrary fucceffion of colours, beginning with red and ending in white.

50. If this *phænomenon* should be actually perceived by astronomers, we shall have a fufficient direct proof of the different velocities of the coloured rays ; for I fee not to what other cause the *phænomenon* could be rationally afcribed : If it be not, we may conclude that rays of all colours are emitted and reflected with one common velocity.

S E C T.

S E C T. VII.

On the Imperfection of our Knowledge concerning the Inflexions of Light.

51. SIR *Isaac Newton* went a very considerable length in examining the inflexions of light, as well as its reflexions and refractions; but did not bring his inquiry on this head to a conclusion. He tells us, that he intended once, if other business had not called him off, to have made more experiments; not for confirming himself in preconceived opinions, as many do; but for discovering the true manner in which light is inflected, for producing the coloured fringes with black lines between them. He adds, however, some queries which contain hints of what he had gathered on this subject from his own observations, *viz.* that the rays of light differ according to their colour in their degrees of flexibility, and that they are bent several times backwards and forwards with a serpentine motion in passing by the sharp edges of bodies: these thoughts he threw out “in order to incite others to a further search*.”

But,

* *Newt. Opt.* p. 313. see the first two or three queries.

But, so far have his intentions been disappointed hitherto, that few physical writers seem to comprehend distinctly the hints which he has left concerning the manner of inflexions*; and none, as far as I know, has

* When any opaque body is held at the distance of three or four inches from the eye, so that a part of some more distant luminous object, such as the window or the flame of a candle, may be seen by rays passing near its edge: If another opaque body, nearer to the eye, be brought across from the opposite side; the edge of the first body will seem to swell outwards and meet the latter, and, in doing so, will intercept a portion of the luminous object that was seen before.

This *phænomenon* has been rashly ascribed to the inflexion of light, by such as understood not thoroughly the nature of inflexion, nor observed accurately the circumstances of the fact.

Let AB represent the luminous object. (TAB. iii. Fig. 5.) to which the sight is directed, CD the more distant opaque body, GH the nearer, and EF the diameter of the pupil; join ED, FD, EG, FG, and produce them till they meet AB in K, N, M and L. It is plain, that the parts AN, MB of the luminous object cannot be seen. But, taking any point *a* between N and K, and drawing *aDd*; since the portion *dF* of the pupil is filled with light flowing from that point, it must be visible: any point *b* between *a* and K must fill *fF* a greater portion of the pupil, and therefore must appear brighter. Again, any point *c* between *b* and K must appear brighter than *b*, because it fills a greater portion *gF* with light. The point K itself, and every other point in the space KL, must appear with compleat lustre;

advanced one step beyond them. It is surprising, that, before Sir *Isaac Newton*, the world continued so long entirely ignorant of the true theory of light and colours; and it is no less so, that, since he quitted the subject, no further discovery of any moment has yet been made amongst all the philosophical Societies in *Europe*.

52. MANY

lustre; since they send entire pencils of rays EKF , ELF to the eye: and the visible brightness of every point from L towards M must decrease gradually as from K to N : *i. e.* The spaces KN , LM will appear as dim shadowy borders or fringes adjacent to the edges of the opaque bodies. When the edge G is brought to touch the right line KF , the penumbra's unite; and, as soon as it reaches NDF , the above *phænomenon* begins: for it cannot pass that right line without meeting some line aDd drawn from a point between N and K , and, by intercepting all its rays that fell upon the pupil, render it invisible. In advancing gradually to the line KDE , it will meet other lines bDf , cDg , &c. and therefore render the points b , c , &c. from N to K successively invisible; and therefore the edge of the fixed opaque body CD must seem to swell outwards, and cover the whole space NK , while GH by its motion covers MK . When GH is put to a greater distance from the eye, CD continuing fixed; the space OP to be passed over for intercepting NK is less; and therefore, with an equal motion of GH , the apparent swelling of CD must be quicker; which is found true by experience. If ML represents a luminous object, and $REFQ$ any plane exposed to its light; the space FQ will be entirely shaded.

52. MANY ingenious men have bestowed infinite thought and labour on the more complex and astonishing *phænomena* of Nature, without arriving at any certain or definite discoveries; such as earthquakes, thunder and other meteors, magnetism, electricity, vegetation, fermentation and other chemical operations: and the subtilty of those matters will probably continue to elude the search of latest posterity. But, in the simpler, steadier, and more regular subjects, such as light and colours, which are capable of accurate mensuration and mathematical reasoning, a sagacious and industrious observer can hardly fail of making some progress; especially in a branch of the inquiry which is already pushed to a considerable length. Discoveries of this kind are capable of a particular sort of proof which is very beautiful and convincing, from the exact coincidence of the computed effects

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shaded from the rays, and the space FE will be occupied by a *penumbra* gradually darker from E to F: Let now GH continue fixed, and CD move parallel to the plane EF; and, as soon as it is passes the line LF, it is evident, that the shadow QF will seem to swell outwards, and when CD reaches ME so as to cover with its shadow the space RE, QF by its extension will cover FE. This is found to hold true likewise by experiment.

with the real ones, as to quantity. Many instances of this occur in Sir *Isaac Newton*'s writings, and in all mathematical philosophy: such as the calculation of the moon's irregularities; of the tides; of the precession of the equinoxes; of the resistance of fluids; and, in optics, his computation of the dimensions of the rainbow; of the aberration of colours; of the intervals of the fits of reflexion and transmission; and of the coloured rings reflected by thick transparent *speculums*.

WHAT further I have to offer concerning light and colours, consisting chiefly of doubts, difficulties or loose conjectures, shall be proposed under the form of queries.

SECT.

S E C T. VIII.

Queries, consisting of Doubts, Difficulties, and Conjectures, concerning Light, Colours, and coloured Bodies.

QUERY I. Are not the rays, emitted by all sorts of luminous bodies, similar to those of the sun, both as to colour and degrees of refrangibility? And, do not luminous bodies differ from one another only according to the colours which they emit most plentifully, in like manner as opaque bodies are distinguished by the colours of incident light which they reflect in greatest abundance? (See N^o 24, 25, 26, 27, 28, and 29.) But, to make our induction sufficiently strong, ought not experiments to be made with the lights of a greater variety of bodies? And would it not further conduce to the illustration of this question to form, by Sir *Isaac's* method*, a beam of solar light, consisting of such colours and in such proportions as were seen in the lights of salts and burning spirits; and then to observe in it the appearance

* *Newt. Opt. book 1. part 2. prop. 11.*

ance of coloured bodies? Further, are not the intervals of the fits, in rays of any one colour, the same in the same *medium*, from whatever luminous body they are emitted? For, if these intervals were different, would there not be changes in the colours of bodies not to be accounted for by the compositions of the lights with which they are illuminated*?

QUER. II. Do not all luminous bodies, the most languid as well as most bright, emit their lights of any one colour with one determinate velocity; since it is found by experience that they are all equally refracted by the same *medium*? And therefore, does not the different splendor of luminous bodies proceed wholly from the different density of their light at equal distances? And is not this confirmed by the equality of *Bradley's* aberration of light in fixed stars of all magnitudes †? If this be so, the comparative strength of different lights, such as of the sun, moon, a candle, &c. may be easily estimated by finding the greatest distances to which the same opaque body is visible when illuminated by each of them, or the limits beyond

* See Art.

† *Fames's* Abridg. of tranfact. vol. 6. p. 158.

yond which it is invifible to a given eye ; for the denfities of the incident lights are nearly as the fquares of the diftances of thefe limits from the object *. Does not all light move with the fame velocity after reflexion as before ; fince the angle of reflexion is always equal to the angle of incidence ? The exception, made by fome, of electrical light is founded on no lefs a miftake than confounding the luminous body with its light †. But, the beft proof of this propofition is from the coincidence of the computations of the velocity of light, from the equation of the eclipses of *Jupiter's* fatellites and the aberration of the fixed ftars ‡.

QUER.

* Let A and a (TAB. iii. fig. 6.) denote the fame or two equal bodies of the fame colour illuminated with different lights, and B, b , the limits. As we fuppofe the light received by the eye, at thefe points, is juft fufficient to affect it fenfibly and no more, the two lights at thefe different diftances muft be nearly of the fame denfity ; taking therefore in AB a line $A\beta$ equal to ab , the denfity of the light at β muft be, to the denfity of the light at b , nearly as AB^2 to Ab^2 ; and, it is evident, that thefe denfities, at equal diftances, muft be as the whole quantities of light reflected ; and thefe again very nearly as the whole quantities of light incident.

† *Muffchenbroeck's* Elementa Phyfices, late edition, in his chapter on electricity.

‡ *Eames's* Tranfact. vol. vi. &c.

QUER. III. Is light emitted with the same velocity, in whatever *medium* the luminous body be placed? Or, is it not rather emitted with greater velocity in denser *mediums*, and that in proportion to their refractive powers? The same argument from whence we gather in general the equal velocity of light emitted by all sorts of luminous bodies, seems to prove the truth of the latter supposition. For, since rays of any one colour, from the sun and a candle, for instance, are equally refracted by a surface of glass or water, we may conclude, that their velocities in air are equal. Wherefore, if the density of the sun's atmosphere, contiguous to his surface, be different from the density of our lower air, as may be safely presumed, his rays must have been emitted with more or less velocity than that of the candle; otherways, they could not have the same velocity afterwards in any common *medium*: for, the velocity with which any ray is emitted, is, by the laws of refraction, to its velocity in any given *medium*, as the sine of refraction to the sine of incidence, when a ray passes from the

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the *medium* of emission into the given *medium*.

QUER. IV. IF the atmosphere is not much warmed by the passage of the sun's light thro' it, but chiefly by its contact with the heated surface of the globe, as we shewed above (N^o 15.); may we not hence give one very simple and plausible reason, why it is coldest in all climates on the tops of very high mountains; namely, because they are removed to the greatest distance from the general surface of the earth? For it is well-known, that a fluid heated by its contact with a solid body decreases in heat, in some inverse proportion to the distance from the body. But, to have this question fully determined, the temperature of the air in the valley and on the mountain-top must be observed every hour both night and day, and carefully compared together.

QUER. V. FROM what has been laid down in Sect ii. concerning the manner of the action of light in heating bodies, is it not reasonable to suppose that the heat produced by a given number of rays, in an opaque body of a given magnitude, must be greater
when

when the rays are more inclined to one another, than when they are less so? For the direction of the vibrations, raised by the action of the light, whether in the colorific particles or those of an inferior order, will more interfere with one another; from whence the intestine shocks and collisions must increase: besides this, the colorific particles of opaque bodies being disposed in various situations, perhaps, upon the whole, the rays will fall more directly on each, the more they are inclined to one another. Is not this the reason of what has been remarked by philosophers*†, That the heat of the sun's light, collected into a cone, increases in approaching the focus in a much higher proportion than according to its density? That the difference of the angle, in which the rays fall on any particle of a given magnitude placed at different distances from the focus, is but small, is no proof that the *phænomenon* cannot be ascribed to it; since we know not in what high proportion one or both the circumstances now mentioned may operate. However, that

* *Boerhaave*, Element. chemic. de igne.

† *Musschenbr.* Elementa Physicæ, § 1040.

that it proceeds not from any unknown action of the rays upon one another, as has been insinuated*, is evident from this, that each particular ray, after passing thro' the focus, preserves its own colour and its own direction, in the same manner as if it were alone.

QUER. VI. MAY it not be inferred, that the component parts of opaque bodies are greater than those of transparent ones, as theory requires†, from this simple observation, that the former, such as metals, stones, woods, &c. when broken transversely, shew a visible roughness and inequality at the fracture; whereas the latter, such as glass, chrystal, gems, ice, &c. appear as smooth, almost, as when they are polished?

QUER. VII. Do not *Newton's* experiments with the island and rock chrystal sufficiently prove, that the rays of light have different permanent properties in their different sides, relative to these two bodies? Must we not therefore conceive each particle of light to preserve its position invariably while it moves forward, at least so as not to revolve round

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* *Musschenbroeck's Elementa physices*, § 1040.

† *Newton's Opt.* Book 2. part 3. prop. 4.

its center perpendicularly to the direction of its motion? Would it not be proper to try how light is inflected in passing closely by the several angles and sides of these fossils?

QUER. VIII. Is it not possible to prove by experiment what Sir *Isaac Newton* takes for granted as a reasonable supposition, that thin transparent plates, of any uniform colour, divided into smaller fragments, would compose a powder of like colour *? And would not this tend to strengthen the analogy between the colours of such plates and those of natural bodies? For this purpose, I have tried to freeze soap-bubbles; but could never make any stand till they were turned to ice, except such as were too thick to have lively colours: however, I doubt not, but, with due care, the thing might be done; especially, if the soap-water, instead of being blown with a pipe into bubbles, were drawn out into a plain plate upon any wooden or metalline frame: for, the sides of a plain surface bearing a greater proportion to its *area*, than a base of a spherical segment to its surface, the frost would be sooner communicated

communicated to the whole water in the former case than in the latter. There is this advantage too in using a plain surface of soap-water, that, before it freezes, the observer may draw out any particular colour or series of colours, which he chuses, to a greater breadth, by stroaking it along with a wet finger. For this reason, amongst others, I have found it a more convenient subject for examining the various orders of colours, than spherical bubbles adhering to a plane. Perhaps, melted rosin might be drawn out into a thin-coloured plate before it hardens ; for I have often blown it into bubbles with a tobacco-pipe till it became coloured. I know no other ways in which the various orders of colours can be preserved for deliberate inspection, but either in a frozen plate of water or rosin, or in the permanent *scoria* that appear on heated metals. I have counted, on the side of a clean-polished copper tea-kettle, the six first orders of colours distinctly and regularly ranged in the same succession in which they appear in the soap-bubbles ; the first order being formed on that part of the kettle that had been least heated.

QUER.

QUER. IX. WHAT else is the inflexion of light towards the fine edges of bodies than a particular case of refraction, in which the rays, after being bent by the attractive power, are carried beyond the refracting surface, and miss entering it, because of its small extent? For, if the surface of the edge be produced, it will meet the inflected rays; and thus the inflexion will become properly refraction. And, in like manner, we may consider the inflexion of light off from the edges of bodies, as a species of reflexion.

QUER. X. Is it not impossible that an animal can see, if the diameter of its eye be much less than the interval between the fits of transmission and reflexion in water, that is, than $\frac{1}{375000}^{\text{th}}$ of an inch?

QUER. XI. THERE are many experiments which shew that a yellow and blue ray mixed, make a green one; a yellow and blue powder, a green powder; and a mixture of rays or paints of all the prismatic colours, a white ray or paint: Now, do not the same experiments equally demonstrate, that the idea of green is a confusion or mixture of the
ideas

ideas of yellow and blue ; the idea of white, a mixture of the ideas of all the colours ; and, in general, the ideas of all compound colours, a mixture of the ideas of their constituents ? In the experiments which Sir *Isaac Newton* performed with the toothed instrument, the component colours are not, indeed, presented to the eye all at once ; yet they follow one another in so rapid a succession, that their respective impressions remain in the eye till they are renewed, and therefore they must affect the mind all at once *. If a piece of paper

* It is in this manner that philosophers explain (*Newt.* (Opt. Quer. 16.) the appearance of a fiery circle, which is made by a burning body whirled about swiftly. We shall here give an account of some other *phænomena* that flow from the same principle.

If a white rod be moved rapidly backwards and forwards with an angular motion, the whole circular space which it runs over will appear whitish ; but not equally so, being faintest and most dilute in the middle, and brighter towards the two sides, which seem to be distinctly terminated with two white rods intersecting each other in the center of rotation. (See TAB. iii. Fig 7.)

The total impression made upon the eye by equal small parts of the sector must be, as the quantity of light emitted from it and the frequency of the returns of the rod to it ; *i. e.* inversely, as the time between the returns of the rod.

Let

paper be daubed all over with small dots of blue and yellow, it will appear green to an eye which is placed at too great a distance to distinguish

Let ABC represent the circular sector, and DC a line bisecting it; the rod always returns to DC after the time of one vibration; and, to any other line EC between DC and AC or AB, the mean time of its return is the same; for it alternately returns in twice the time of describing AE, and twice the time of describing EB; so that two succeeding intervals of its returns are equal to the time of two vibrations: but the intervals of the returns to the lines AC or CB are manifestly equal to the time of two entire vibrations. The brightness of the sector therefore in DC, or any line between DC and AB or BC, must be simply as the quantity of light emitted from equal small portions of the sector; that is, in the inverse proportion of the velocities of the rod when in these lines. It is plain from this, that the sector must be *incomparably* brighter in AC and BC, where it rests, than any where else, notwithstanding that the intervals of return thither are double; that is, it will appear to be bounded distinctly with a white rod on each side.

If the rod be agitated with small and quick vibrations of its own, by sticking it against some solid body immediately before it is hurried backwards and forwards with the angular motion, the sector appears divided, at equal intervals, by a great many distinct rods, almost as bright as the two lateral ones (TAB. iii. Fig. 8.) resembling the spokes of a spread fan. The reason of which curious *phenomenon* is plainly this; that its angular motion, being alternately in the same and in a contrary direction to its particular vibrations, is alternately accelerated and retarded or stopt. In the interval, where it is accelerated, the sector must appear very dilute; and, where

tinguish the separate points. In whatever manner sensation be performed, it is certain, that the organs which receive the first impulse from external objects cannot convey to us any ideas, if they, or the impressions made by them, be less than of a certain definite magnitude. A number of things separately intangible, if joined together, may be felt by the touch; A certain number of invifible points become fufficient to affect the fight by their united rays; and a certain number of founds too small to be heard feperately, at laft form an audible found *.

QUER.

it is greatly retarded or brought to reft, muft appear very luminous or divided by white rods, for the fame reafon that they appear at the fides.

* Some Sceptics have difputed againft the endless divifibility of quantity, becaufe the imagination foon arrives at a *minimum*; alledging from thence, that our idea of extenfion involves the notion of indivifibles, and is as it were compounded of them. Nothing corporeal can be imagined or conceived at all which is not conceived as *feen*, *handled*, or otherways fenfibly perceived. Imaginative ideas are nothing elfe than tranfcripts or images of fenfations, and therefore muft be limited by the fame bounds and in the fame manner as fenfation. Now the *minimum fenfibile* is rather in all cafes a confufed, indiftinct and uncertain tranfition from *perceivable* to *not perceivable*, than the clear perception of a point indivifible in magnitude; for its magnitude depends

QUER. XII. SINCE bodies derive their colours from the original and immutable qualities of those rays which they reflect most copiously, ought they not to appear of the same colour, whether viewed at the greatest or least distances? Whence is it therefore, that the planets whose solid parts are probably covered with vegetables, and must therefore reflect a great superiority of green

on the lustre of the object. That nothing can be conceived or imagined which is less than a certain bulk, is no more an argument against the endless divisibility of quantity, than that nothing can be felt or seen below that size; which, it is evident, from every magnifying glass and from every different distance of an object, depends not at all on the constitution of the thing perceived, but on that of the perceiver, or the means and circumstances of his perception.

Nor, tho' it were granted that the *minimum visibile* is distinctly seen as an indivisible point, would it follow, that the idea of extension, received by sight, is made up of the ideas of indivisibles; for we receive the idea of extension by that motion of the eye which is necessary to direct its *axis* to different objects or parts of an object: and, it is well known, that the generation of quantity by motion is preferred by the best writers, for this very reason, that it necessarily excludes the notion of indivisibles. It should be remembered likewise, that a visible object is not divided by the eye into a number of contiguous *minima visibilia*; for, to whatever mathematical point in the object the eye is directed, a *minimum visibile* may be seen there by means of a certain portion of the object immediately surrounding it.

green rays, appear almost intirely white when viewed from the earth? May not this be accounted for, in the same manner as the change of colour observable in earthly objects seen thro' a great tract of the atmosphere? A mountain covered with the freshest verdure, at the distance of twelve or fifteen miles, looks blueish; and at twenty or thirty, especially if the air be thickened, degenerates into a dim white, so that one can hardly distinguish it from the clouds that skirt the horizon. With respect to the primary planets, it may be likewise answered, that perhaps we see them chiefly by light reflected from the air and vapours that surround them.

QUER. XIII. Why is it so hard to distinguish green bodies from blue by candle-light?

QUER. XIV. Whence proceeds the blueness of the sky? Since it is certain that no body assumes any particular colour, but because it reflects one sort of rays more abundantly than the rest; and since it cannot be supposed that the constituent parts of pure air are gross enough to separate any colours of themselves; must we not conclude, with

Sir *Isaac Newton**, that the violet and blue-making rays are reflected more abundantly than the rest, by the finer vapours diffused thro' the atmosphere whose parts are not big enough to give them the appearance of visible opaque clouds? Do not those who say †, that the ethereal blue proceeds from the mixture of the sun's white light reflected faintly by the atmosphere with the perfect blackness of the celestial space behind, revive, without any necessity, the antient confused notion, that all colours may be formed by certain compositions of light and shade? Altho' the atmosphere reflects more blue rays than what go to the formation of perfect white, it is easy to conceive how coloured bodies, illuminated by it, may not be sensibly tinged with blue. Let us suppose, that the atmosphere reflects $\frac{1}{4}$ more of blue rays than of the other colours, and that vermilion reflects $\frac{19}{20}$ of the red rays incident upon it, and $\frac{1}{20}$ of every other colour; then, it is clear, that the red rays, reflected by the vermilion, will still exceed the blue reflected by it, as 19 exceeds $1 + \frac{1}{4}$: so that

* Opt. book 2. part 3. prop. 7.

† Nature displayed, vol. iv. And *Muschen. Phys.* § 1402.

that the purity of its red colour will not be sensibly impaired. But, to shew that, in proper circumstances, the blueish colour of sky-light may be seen on bodies illuminated by it, as it is objected should always happen *; expose to the sun-beams, on a clear cloudless day, a sheet of white paper, and place on it any opaque body; you will perceive that the space of the shadow, which is illuminated only by the sky, appears remarkably blueish, compared with the rest of the paper which receives the sun's direct rays. If certain white and black paints mixed together produce blue, it is because the black is not perfect shade, but a dark blue or purple † ‡. Any mixture of whiteness and true black can only form a fainter white or grey, which has no more affinity with blue than with red or any other colour.

QUER. XV. Is not the opinion which Sir *Isaac Newton* seems to have had ||, and, since him, the generality of philosophers, concerning the cause of the various colours reflected

* *Muschen. Phys.* § 1403.

† *Ibid.* § 1172.

‡ *Newt. Opt.* book 2. part 3. prop. 7.

|| *Opt.* book 2. part 3. prop. 5. near the end.

flected by the clouds at sun-rising and setting,
 liable to great difficulties? For, why should
 the particles of the clouds become, at that
 particular time and never at any other, of
 such magnitude as to separate these colours?
 And why are they rarely, if ever, seen tin-
 ctured with blue and green, as well as red,
 orange and yellow? Is it not more credi-
 ble that the separation of rays is made in pas-
 sing thro' the horizontal atmosphere? and
 that the clouds only reflect and transmit the
 sun's light, as any half transparent colourless
 body would do in their place? For, since the
 atmosphere, as was said in the last query,
 reflects a greater quantity of blue and violet
 rays than of the rest, the sun's light, trans-
 mitted thro' it, ought to draw towards yel-
 low, orange, or red; especially when it
 passes thro' the greatest tract of air: accord-
 ingly, every one must have remarked, that the
 sun's horizontal light is sometimes so deeply
 tinctured, that objects directly illuminated by
 it appear of a high orange or even red; at
 that instant, is it any wonder that the colour-
 less clouds reflect the same rays in a more
 bright and lively manner? It is observable,
 that the clouds do not commonly assume
 their

their brighter dyes till the sun is some minutes set ; and that they pass from yellow to a flaming golden colour ; and thence, by degrees, to red ; which turns deeper and deeper, tho' fainter, till the sun leaves them altogether. Now, it is plain, that the clouds, at that time, receive the sun's light thro' a much longer tract of air than we do at the instant of setting, perhaps by the difference of a hundred miles or more ; as may be computed from their height or the duration of their colours. Is it not, therefore, natural to imagine, that, as the sun's light becomes always somewhat yellowish or orange in passing thro' the depth of the atmosphere horizontally, it ought to incline more and more from orange towards red, by passing thro' a still greater length of air ; so that the clouds, according to their different altitude, may assume all the variety of colours, observed in them at sun-rising and setting, by barely reflecting the sun's incident light as they receive it ? I have often observed with pleasure, when in *Switzerland*, that the snowy summits of the *Alps* turn more and more reddish after sun-set, in the same manner as the clouds. What makes the
same

same colours much more rich and copious in the clouds, is their semi-transparency joined with the obliquity of their situation.

Does it not greatly confirm this explication, that these coloured clouds immediately resume that dark leaden hue which they receive from the sky as soon as the sun's direct rays cease to strike upon them? For, if their gaudy colours arose, like those of the soap-bubble, from the particular size of their parts, they would preserve nearly the same colours, tho' much fainter, when illuminated *only* by the atmosphere. About the time of sun-set or a little after, the lower part of the sky, to some distance on each side from the place of his setting, seems to incline to a faint sea-green, by the mixture of his transmitted beams, which are then yellowish, with the ethereal blue: at greater distances, this faint green gradually changes into a reddish brown; because the sun's rays, by passing thro' more air, begin to incline to orange: and, on the opposite side of the hemisphere, the colour of the horizontal sky inclines sensibly to purple; because his transmitted light which mixes with the azure, by passing thro' a still greater length
of

of air, becomes reddish ; as we have said above.

To understand distinctly why the sun's rays, by passing thro' a greater and greater quantity of air, change by degrees from white to yellow, thence to orange, and lastly to red, we have only to apply to the atmosphere, what Sir *Isaac* says (Book I. of his Optics, part 2. prop. 10.) concerning the colour of transparent liquors in general.

Is it not the same coloured light of the rising and setting sun which tinctures the clouds, that, being thrown by the refraction of the atmosphere into the earth's shadow, gives the moon sometimes, in total eclipses, the obscure reddish colour of brick? As the rays which pass thro' the greatest tract of air, become reddish ; those which pass thro' the least, yellowish ; and the intermediate ones, orange : the red must converge fastest into the shadow ; after them, the orange ; and lastly, the yellow : so, that the whole space of the earth's shadow, from the point of the cone to about femidiameters from the earth, being filled with a faint light, whose colours verge always more to red in approaching the earth ; the colour of the
moon,

moon, in total eclipses, must needs vary likewise, according to her distance from the earth at the time of observation; and, if I mistake not, be always more inclined to red at entering and leaving the shadow, than in the middle. Let Astronomers determine, whether the *phænomena* agree with this theory. It is not surprizing, that this refracted light is very faint and obscure at the distance of the moon; since its mean density there, will be as much less than the density of the light of the setting sun, as the annular space of the lower air thro' which it passes, drawn into the moon's horizontal parallax, is less than the area of a great circle of the earth drawn into four times the excess of the horizontal refraction of the atmosphere above the same parallax; that is perhaps as 1 to or more.

QUER. XVI. I have observed, when at sea, that, tho' I pressed my body and head firmly to a corner of the cabin, so as to be at rest in respect of every object about me, the different irregular motions of the ship, in rolling or pitching, were still discernible by the sight: How is this fact to be reconciled to optical principles? Shall we conclude, that the eye, by the sudden motions of the vessel,

vessel,

vessel, is rolled out of its due position? Or, if it retains a fixed situation in the head, is the perception of the ship's motion owing to a vertigo in the brain, a deception of the imagination; or to what other cause?

QUER. XVII. HAS not gold been reduced, by beating, to a degree of thickness little exceeding that which must be ascribed to its colorific parts, according to Sir *Isaac Newton's* theory? But, how can it cohere into a continuous leaf, so as to leave no visible pores, unless there be many of its component particles contained in its thickness?

QUER. XVIII. WHEN one looks steadfastly at *Sirius* or any bright star not much elevated above the horizon, its colour seems not to be constantly white, but appears tinged, at every twinkling, alternately with red and blue: To what is this appearance owing? Is not the separation of colours by the refraction of the atmosphere too small to be perceived?

QUER. XIX. BODIES become black by burning; because they are reduced* into very small parts: but, whence is it, that

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most

* *Newt. Opt.* Book iii. part 2. prop. 7.

most bodies, when further burned to ashes, assume a grey or whitish colour?

QUER. XX. SINCE the cause of blackness in bodies is the smallness of their transparent parts, which renders them incapable of reflecting any colour; how can black bodies, solid or fluid, be at the same time opaque? Can light be finally stifled by the refractive powers of the particles alone? or, ought it not rather to make its way thro' the body, if there be no reflexion, without any sensible loss, altho' the several rays might issue in various directions? And, may it not be demanded, in like manner, concerning all coloured opaque bodies, How all sorts of light can be stifled and stopt within a body, whose internal parts are fitted to reflect only one or two colours, and transmit all the rest?

QUER. XXI. IF the parts and pores of pellucid bodies be much less than the least interval between the fits of reflexion and transmission; it is plain, that rays of light, entering a part or pore in a fit of transmission, will not be reflected at its back surface: and thus it may be understood, how all rays that enter the first surface of a transparent body continue to be transmitted thro' its substance

to the greatest distances, *viz.* if the rays are always put into a new fit of *most easy* transmission at entering every new pore or particle. But is not that supposition contrary to what Sir *Isaac* teaches elsewhere; That the fits of reflexion and transmission continue to return at equal intervals, after a ray has entered a transparent body, and are thus regularly propagated to the greatest distances*? And, if this be true, how can the rays be transmitted to any sensible distance, since they must often arrive in fits of easy reflexion at the common surfaces of pores and particles? But, altho' it could be understood by the doctrine of the fits in light why there is no reflexion from the interior parts of water and other pellucid *mediums* †, does not the rectilinear transmission of light thro' these bodies in all directions, and consequently in all degrees of obliquity, to their internal parts, prove, that these parts, upon account of their minuteness, lose their powers of refraction as well as reflexion? And to what known property of light or bodies can this be attributed?

QUER.

* *Newt.* Opt. Book ii. part 2. prop. 12.

† *Ibid.* Book ii. part 3. prop. 4.

QUER. XXII. IF the fits are produced by an alternate acceleration and retardation of the particles of light, some of the particles, which are swift enough to be transmitted at the first surface of a transparent *medium*, must overcome the reflecting power more easily than others; namely, those that happen to be in their point of greatest celerity or nearest to it: Now, must not rays that are moving with different velocities be differently bent from their course, as we argued above with respect to simple-coloured rays, by the same refractive power? Why therefore is not every beam of light, homogeneous or heterogeneous, diffused by refraction into innumerable rays, according to the respective velocities with which they entered the refracting surface? Is it a sufficient answer to this query, That rays which are farthest from their point of greatest swiftness will be most bent in a direction contrary to that of refraction, by the reflecting power, and will therefore only return to the direction of swifter rays by a greater degree of refraction?

QUER. XXIII. Sir *Isaac Newton* justly argues, that light must be reflected at a distance from bodies; because the most polished surface,

face, being extremely rough and uneven in respect of the particles of light, would disperse them indifferently in all directions, if they rebounded from it by striking: But, will not the like difficulty still remain, *viz.* how light can be reflected or refracted regularly by the best-polished surface, if the power of the body proceeds from an attraction or repulsion belonging to each physical point? It might be perhaps supposed, that the repulsive power produces reflexion at a distance so great, in respect of the inequalities that are left in polished bodies, that the direction of force, resulting from their joint action, may be very nearly perpendicular to the general surface of the body; and this might tend to account for the regular reflexion from the anterior surface of a denser *medium*. But, will this supposition suffice for explaining the regularity of refraction, and of reflexion, from the posterior surface of a denser *medium*: in both which cases, the light must actually enter the pores of the attracting body, and therefore approach much nearer to one inequality than another; since the pores, by which it enters, are certainly much less than those inequalities? In
water

water and other transparent liquors, this must certainly be the case, if their globular particles touch one another, as is commonly concluded from their incompressibility: for, as a number of spheres laid together leave no rectilinear passages between them, the transmitted light must pass thro' the component particles; and therefore the pores, by which it enters, must be much less than the whole hemispherical surfaces of the particles which evidently constitute the inequalities of the general surface of the liquor*.

QUER. XXIV. How does light preserve its rectilinear course in passing thro' air, *æther* and other elastic fluids? Will not the difficulty still continue, whatever subtilty or rarity is ascribed to these *mediums*; since the powers from whence their elasticity arises, must prevail thro' all the free spaces that intervene their particles? Must we not, therefore, suppose, that the rays of light are not
subject

* We are certain, that the inequalities of a craggy rock or rough wall are much greater than the particles of air or their distance from one another, by which their repulsive powers are probably terminated (*Newt. Princip.*): Why is sound, therefore, reflected so regularly from such bodies, that the echo is faintly heard, except at an angle of incidence equal to the angle of reflexion?

subject to these repulsive powers, tho' they pass thro' the sphere of their action? Does not the refraction of light towards the perpendicular, out of the celestial spaces into air, even prove that it is attracted by the particles of air? Would it not be extravagant and incongruous beyond measure, to imagine the *æther* so subtile, in respect of light, that, tho' it be driven out of the way by the rays, as air is by common projectiles, it is not capable of retarding them sensibly in their motion from the most distant fixed stars to our eye? Do not these and many other difficulties, in the physical part of Optics, whose solution is sought for in vain from any principles hitherto discovered, shew the necessity of extending our views and enlarging our stock of principles by further experimental inquiry? Such objections are not to be considered as demonstrations of the falsehood of our present theory; but as proofs of its narrowness, partiality and imperfection.

DES CARTES, contenting himself with a superficial and inaccurate knowledge of the laws of impulse, vainly dreamed, that he had got possession of the universal cause
from

from whence all effects in Nature are derived ; when, in truth, he was unable to deduce from them the simplest cases of collision. Many in this age, who write and speculate on physical subjects, seem to fall into a like error ; while they employ their whole study in endeavouring to reconcile all *phænomena* with the new principles discovered by Sir *Isaac Newton* : and, when they find, to their mortification, that this will not always succeed ; *phænomena* must be disguised, and Nature tortured, to hide their ignorance. From the lazy method of philosophizing in the closet, among books and diagrams, there never arose, there never will arise, any discovery of consequence : Great inventors usually understand the extent of their own principles too well, to leave much of the application of them to others.

THE discovery of the different refrangibility of the rays, was an inestimable addition to natural knowledge ; as it serves, at once, for explaining innumerable *phænomena* in Nature which flow from it as immediate and necessary consequences : and, if it shall be demonstrated by the observation proposed in N^o 49. that the differently-coloured rays really

really move with different velocities, our theory of light will be still farther improved ; as the different refrangibility can be thence mechanically explained.

THE whole system of Nature is one immense series of causes and effects, whose beginning and end are equally hid in the depths of infinity. Only a small, a very small portion of it, comes under our immediate observation ; being exposed alike to the sight and other senses of all mankind. Almost every *phænomenon* is, at once, the cause of manifold effects ; and one effect, among many, of a superior cause. The business of Science is to extend our views, by unfolding the latent causes which exist in Nature ; and thence explaining their manifest effects. The discovery of one such real cause, unknown before, if it be of general or very extensive influence, as that of universal gravity, is to be esteemed a great advancement of natural philosophy. To undervalue such a discovery, as some have done, because the cause of that cause cannot yet be assigned, is highly absurd : since the same objection must for ever ly against

all causes, except *primary* ones; which are certainly removed far beyond the reach of human inquiry. The proper office, and highest boast of true philosophy, is, to bring us still nearer to the DEITY, by leading us upwards, step by step, in the mighty *scale* of Nature,

ART.

Fig 1.st

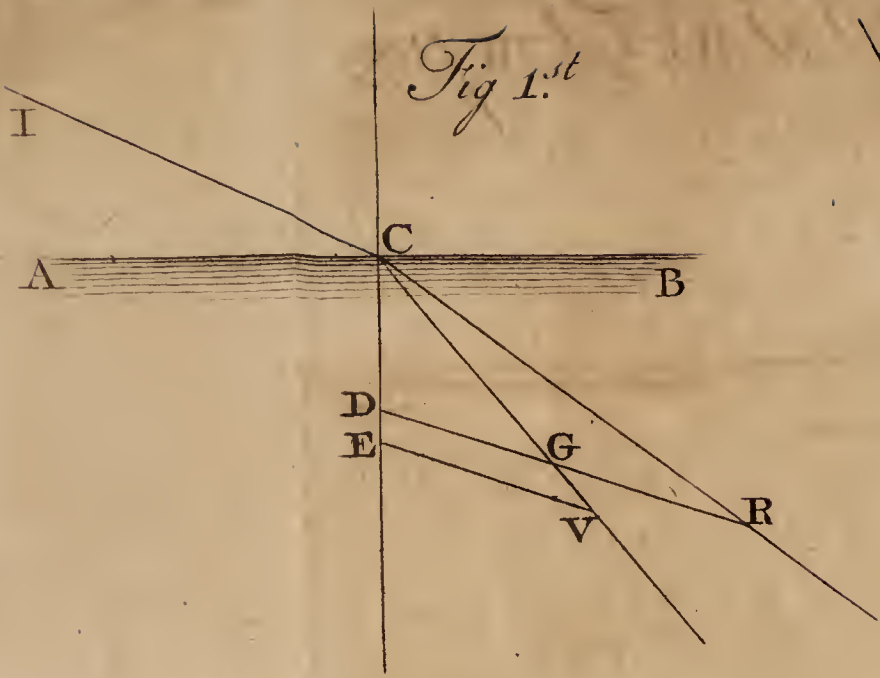


Fig. 2.^d

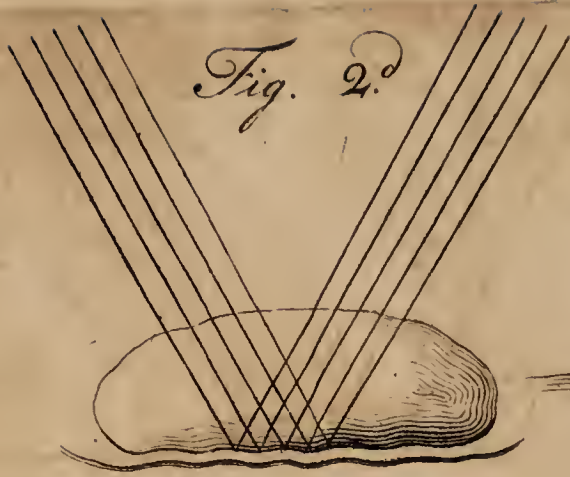


Fig. 3.^d

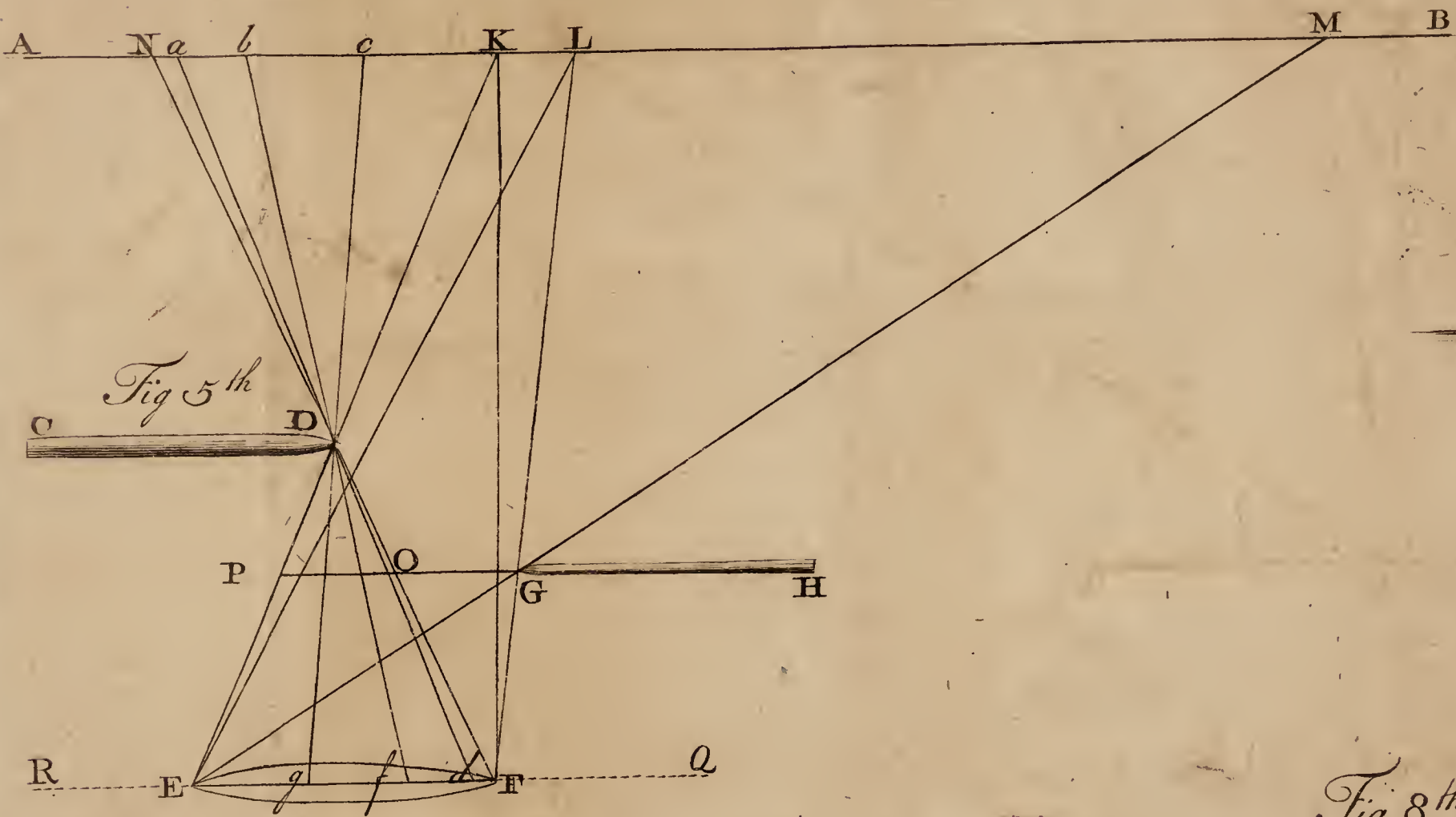
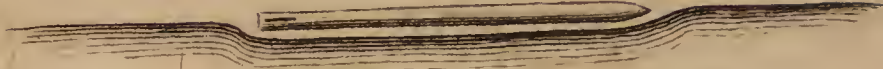


Fig 5.th

Fig. 4.th

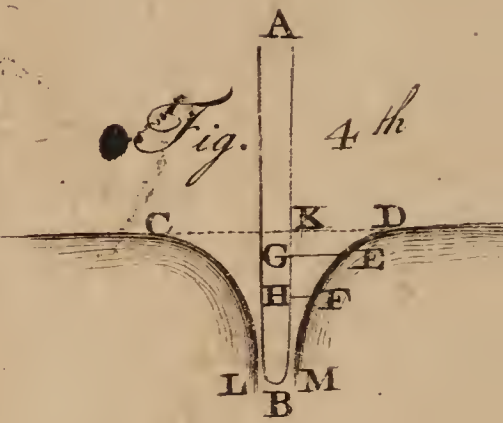


Fig 7.th

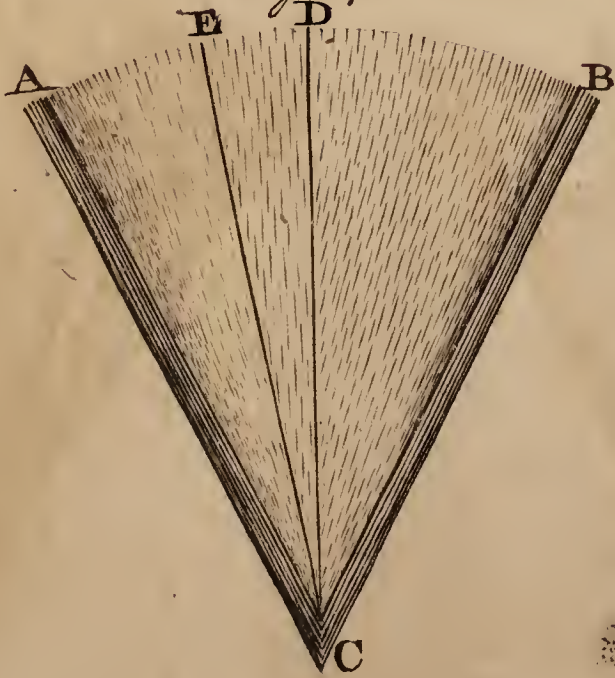


Fig 8.th

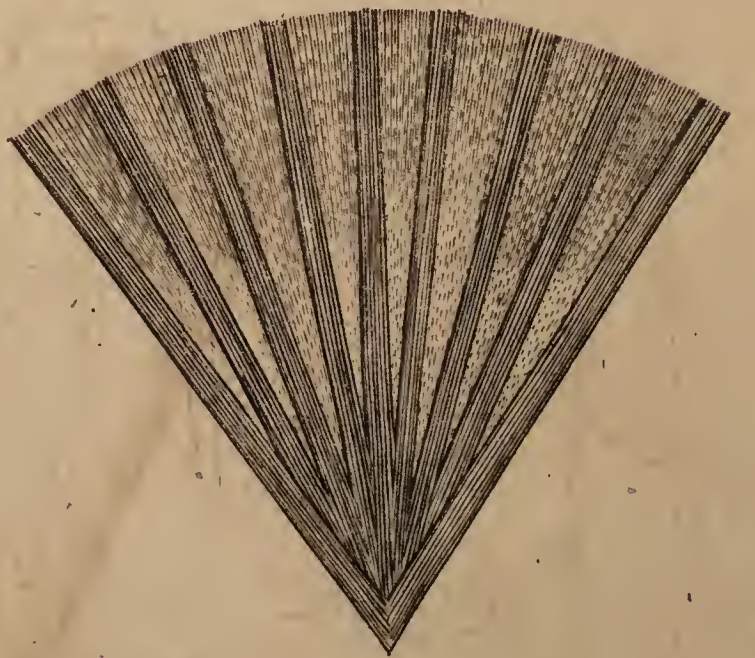
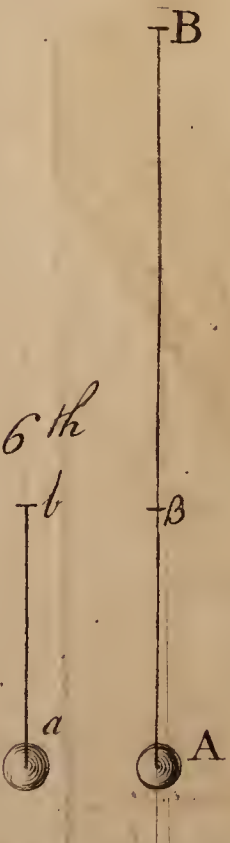


Fig 6.th





ART. V.

*An easy Method of computing the Parallaxes
of the Moon**; by——

T A B. IV. *fig. 1.* 1. IF from \odot , the center of the sun, a right line $\odot W$ be carried round, always touching the earth's surface, this line will form a conical surface; which, being cut, by a plane passing through the center of the earth, at right angles to the line joining the centers of the sun and earth, the section, so made, is the disk of the earth.

2. THE same conical surface will cut off a circular portion of the moon's sphere, *w n e s*; within which, any arches, intercepted by lines drawn from the center of the sun and extended to the disk of the earth, will be nearly in the same proportion to one another, as the respective distances intercepted on the disk.

3. HENCE, *c* being the center of the circular portion, *w c e* the ecliptic; if the semidiameter of the disk be expressed by the number of seconds in the arch *c w* or *c e* = horizontal parallax

* November 6th, 1755.

rallax of the moon from the sun, every line drawn on the disk will be expressed by the seconds of its correspondent arch of the moon's sphere.

4. LET C be the center of the disk, $W E$, $N S$, the projections of $w e$ the ecliptic, and $n s$ a circle of latitude; $W N E$ being the upper or northern semicircle, and $W S E$ the lower or southern. Let V be the place of any given vertex on the disk, and v the correspondent point in the moon's sphere; $V A$, $V B$, perpendiculars to $N S$, $W E$, and $v a$, $v b$ their correlatives. If the point v be the true place of the moon, *i. e.* if the visible places of the sun and moon be the same, then will $V A$ be the par. lon. $\bowtie \hat{a} \odot$, and $V B$ the par. lat. to a spectator at the point V on the disk, or at the given vertex on the surface of the earth. For the par. lon. $\bowtie \hat{a} \odot$ is the difference of the vis. lon. $\bowtie \hat{a} \odot$ observed at V and C : but the vis. long. $\bowtie \hat{a} \odot$ observed at C , is the same as if seen from A ; therefore the par. lon. $\bowtie \hat{a} \odot$ is equal to the difference of the vis. lon. $\bowtie \hat{a} \odot$ observed at V and A , which is equal to the arch $v a$ the measure of $V A$. In like manner, $V B$ may be proved to

to be the par. lat. $\propto \odot$; the vis. lat. being the same when observed at B or C.

5. IN any other position of the moon, if its distance from the point v exceeds not one degree, which it can never do in the time of an eclipse; the parallaxes, to a spectator at the point V of the disk, will continue nearly the same as before, without any sensible alteration. Let l be the place of the moon, ld , lf , perpendiculars to cn , ce , and let fl , va , be produced till they meet in the point g , and let ld meet with vb in the point t ; the vis. lon. $\propto \odot$, will be nearly the same at the points g and l . Therefore the vis. long $\propto \odot$, observed at V, is the angle under which the arch vg is seen from that point. But the arch vg will be seen nearly under the same angle from the points V and C; and consequently it is the measure of the vis. lon. $\propto \odot$. But the true lon. is $dl = ag$: therefore $vg - ag = va$ is the par. lon. $\propto \odot$. Again, $ad = lg =$ vis. lat. $\propto \odot$; and $cd = fl =$ tr. lat.: therefore $vb = ac = ad + dc =$ par. lat. $\propto \odot$.

6. BUT these parallaxes va , vb , and the vis. lon. and lat. vg , vt , suppose the spectator at the point V on the disk; whereas his
true

true place is at the correspondent point of the surface of the earth; consequently, the vis. lon. and lat. $\propto \odot$, vg , vt , must be increased in the ratio of the distance of the point v from the spectator's place on the surface, to its distance from the point V on the disk. But, as the horizontal semidiameter of the moon should likewise be increased in the same ratio, it will equally answer the purpose of finding the times and phasis of an eclipse, to let these continue unaltered, and to diminish the sun's apparent semidiameter in the same ratio.

7. LET CP be the axis of the earth projected on the disk, FMD the ellipse into which the parallel of the given place is projected, DF its greater axis, OM its lesser semiaxis, VG perpendicular to CO ; MH , GK , perpendiculars to CW , and MI , GL , at right angles to CN ; and, for shortening the rules, put CW , the semidiameter of the disk, or the horizontal parallel $\propto \odot$, equal to the radius of the tables of sines and tangents.

8. FOR computing the parallaxes VA , VB , it will be convenient first to suppose the sun in the meridian, and the place of the vertex

tex

tex at M; then to compute the variation of parallaxes, for the given time, from noon.

9. THE meridian par. lon. $\Delta \hat{a} \odot$ is MI or CH; and the meridian par. lat. $\Delta \hat{a} \odot$ is MH or CI: for finding which the requisites are,

Sine	}	of the lat. of the place,	}	To be found from the Tables.
Cof.				
Sine	}	of the declination of the sun,		
Cof.				
Sine	}	of PCN = complement of the meridian angle,		
Cof.				

$$OC = \frac{S, \text{ lat. pl. } \times \text{ cof. decl. } \odot}{R} \left\{ \begin{array}{l} \text{North} \\ \text{South} \end{array} \right\} \text{ if the lat. } \left\{ \begin{array}{l} \text{North.} \\ \text{South.} \end{array} \right\} \text{ pl. be}$$

$$OM = \frac{\text{Cof. lat. pl. } \times S, \text{ decl. } \odot}{R} \text{ of a contr. spec. to the decl. } \odot.$$

$$CM = \left\{ \begin{array}{l} \text{Diff.} \\ \text{Sum} \end{array} \right\} \text{ of } OC \text{ and } OM, \text{ if they } \left\{ \begin{array}{l} \text{the same} \\ \text{different} \end{array} \right\} \text{ Species.}$$

$$\text{Merid. par. lon. } \Delta \hat{a} \odot = MI = HC = \frac{CM \times \text{cof. merid. ang.}}{R}$$

$$\left\{ \begin{array}{l} \text{East} \\ \text{West} \end{array} \right\} \text{ from the } \left\{ \begin{array}{l} \text{Summer} \\ \text{Winter} \end{array} \right\} \text{ to the } \left\{ \begin{array}{l} \text{Winter} \\ \text{Summer} \end{array} \right\} \text{ solstice } \left\{ \begin{array}{l} \text{North.} \\ \text{South.} \end{array} \right\} \text{ if CM is}$$

$$\text{Merid. par. lat. } \Delta \hat{a} \odot = MH = IC = \frac{CM \times S, \text{ merid. ang.}}{R} \left\{ \begin{array}{l} \text{of a contr.} \\ \text{spec. to CM.} \end{array} \right.$$

10. THE variations from the merid. parallaxes for the given hour from noon, may be conceived to be made up, each of two parts; which are the projections of MG and GV upon CW for the lon. and upon CN for the lat.: the former being HK, K B;

K B; and the latter I L, L A. For finding which, besides the foregoing requisites, there is required

Sine
Verf. Sine } of the degrees from noon.

$$MG = \frac{OM \times \text{verf. S, deg. à noon}}{R}$$

$$GV = \frac{\text{Cof. lat. pl.} \times \text{S, deg. à noon}}{R}$$

Variation of the PAR. LON. D à ☉.

$$\text{1st PART} = \frac{MG \times \text{cof. merid. ang.}}{R}$$

West } when the sun is in { ♄ ≈ ✕ ♀ ♂ II.
East } { ☿ ♁ ♃ ♅ ♆ ♇

$$\text{2d PART} = \frac{GV \times \text{S, merid. ang.}}{R} \begin{cases} \text{East AM.} \\ \text{West PM.} \end{cases}$$

Variation of the PAR. LAT. D à ☉.

$$\text{1st PART} = \frac{MG \times \text{S, merid. ang.}}{R} \begin{cases} \text{South} \\ \text{North} \end{cases} \text{contrary to the decl. } ☉.$$

$$\text{2d PART} = \frac{GV \times \text{cof. merid. ang.}}{R}$$

Forenoon { South } when the sun is in { ♄ ≈ ✕ ♀ ♂ II.
 { North } { ☿ ♁ ♃ ♅ ♆ ♇

Afternoon { North } when the sun is in { ♄ ≈ ✕ ♀ ♂ II.
 { South } { ☿ ♁ ♃ ♅ ♆ ♇

11. BY art. 6. the part to be subtracted from the apparent semid. \odot , must bear the same proportion to the whole, as the distance of the vertex on the surface of the earth from its point on the disk, to the distance of the points V, v ; which is nearly the same with the semidiameter of the moon's orbit, and may always be expressed by a constant number, *viz.* that of the 2ds of a degree equal to the radius of a circle. The distance of the vertex from its point on the disk may be conceived to be made up of two parts, *positive*, or *negative*: the first of which is the perpendicular distance of the centre of the given parallel circle from the plane of the disk; the second part is the perpendicular distance of the given vertex from a plane passing thro' the foresaid centre, and parallel to the disk.

$$\text{1st PART} = \frac{S, \text{ lat. pl.} \times S, \text{ decl. } \odot}{R} \begin{cases} \text{Pos.} \\ \text{Neg.} \end{cases} \begin{cases} \text{when the lat. pl.} \\ \text{and decl. } \odot \text{ are} \end{cases}$$

of $\begin{cases} \text{the same} \\ \text{different} \end{cases}$ Species:

$$\text{2d PART} = \frac{\text{Cof. lat. pl.} \times \text{cof. decl. } \odot \times \text{cof. deg. à noon}}{R R}$$

Positive } if the given hour from noon be $\begin{cases} \text{less} \\ \text{more} \end{cases}$ than 6.
 Negative }

12. IT would be easy, from the foregoing rules, to construct tables of the parallaxes for any given latitude. Such tables would be sufficiently exact, if calculated for every third degree of the sun's longitude, and every quarter of an hour from noon to 6 hours, assuming 10000 for radius, or the horiz. par. $\text{D à } \odot$. The equation to be subtracted from the apparent semid. \odot needs only be calculated for every 10th degree of longitude, and every half hour from noon to 6 hours, taking the mean semid. \odot .

13. IF the given time from noon be more than 6 hours, subtract it from 12 hours, and the lon. \odot from 12 signs; and, for the remaining time and longitude, seek the intire parallax, and the equation for the semid. \odot , changing its sign.

14. SUCH tables, tho' constructed for a particular latitude, may easily be made to serve for any other latitude. For the variations from the meridian parallaxes are always proportional to the cosine of latitude. And the meridian parallax, whether of lon: or lat. is made up of the sum or difference of two parts; the first of which is proportional to the sine, the second to the cosine of lat.

These

These two parts for the tabular latitude are found thus. To the given lon. \odot add 6 signs; and take out the merid. parallax answering to each longitude. Half the sum of these gives the 1st part, and half their difference the 2d part, to be added to the first when the merid. par. for the given lon. \odot is greater than the other, and to be subtracted when less. In like manner, the equation for the semid. \odot consists of two parts; the 1st of which is proportional to the sine, the 2d to the cosine of lat. The 1st part is the equation for the given lon. \odot , and 6 hours from noon; and this subtracted from the equation for the given lon. \odot and hour from noon, gives the 2d part.

15. THE same rules will serve for finding the moon's parallaxes from a fixt star or planet. If the semid. of the planet have no sensible magnitude, there will be no place for the correction mentioned in art. 6. But, as the requisites could not be found from the common tables, if the star has latitude, it will be convenient, first, to suppose it has none, and afterwards to apply a proper correction. Besides, the proceeding in this way will make the tables already described to be
of

of the same use as in solar eclipses, substituting the lon. * for the lon. ☉, and, for the hour from noon, the equatorial distance of the star's ecliptic place from the meridian, converted into time at the common rate of 15 degrees to an hour.

16. IF the star have latitude, then the true lon. and lat. \mathfrak{D} δ * are measured by arches, from the center of the moon, perpendicular to a circle of latitude, and to a great circle at right angles to it, both passing thro' the star. Hence,

As radius to cos. true lat. \mathfrak{D} , so tr. lon. \mathfrak{D}

δ * to the same projected. And,

As cos. tr. lon. \mathfrak{D} δ * to radius, so tan.

tr. lat. \mathfrak{D} to the same projected : or,

without any sensible error, so tr. lat. \mathfrak{D}

to the same projected.

17. To find the correction of the parallaxes for the latitude of a star: let CW (fig. 2.) be the ecliptic on the earth's surface, N its pole; CN a circle of latitude thro' the star; W its pole; V the given vertex, CD, the lat. *; WVA a great circle thro' the vertex V, meeting with the circle of
of

of latitude in A, WD another great circle ; VB, Vb, arches perpendicular to WC, WD. Then WA is a quadrant, and the sine of VA is the par. lon. $\propto \delta^*$, whether the star has latitude, or not: the sine of VB is the par. lat. if the star be in the ecliptic; but, if it is in the circle WD, the par. lat. will be the sine of Vb; which may be found from the parallaxes of lon. and lat. given for the ecliptic place of the star: thus,

$$S, WV = \text{Cof. } VA = \sqrt{R + \text{par. lon.} \times R - \text{par. lon.}}$$

$$S, VWB = \frac{R \times S, VB (\text{par. lat.})}{S, WV}$$

$$VWb = VWB \left\{ \begin{array}{c} + \\ - \end{array} \right\} CWD, \text{ if the lat } * \text{ be } \left\{ \begin{array}{c} \text{South.} \\ \text{North.} \end{array} \right.$$

$$S, Vb = \text{par. lat. } \propto \delta^* = \frac{S, WV \times S, VWb}{R}$$

18. Since the radius of the tables of sines and tangents was all along taken for the horiz. par. $\propto \delta^\odot$ or $*$; the parallaxes and equation found must be altered proportionally.

E X A M-

EXAMPLES.

*The beginning of the solar eclipse, July 14, 1748. was observed by Doctor
BRADLEY, at Greenwich, to be 9^h 4' 30", a. m.*

REQUISITES.

Sine	} lat. of the place,	51 ^h 29' 00"	{	9.8934439
Cof.				
Sine	} decl. N. of the sun,	19 36 00	{	9.7943083
Cof.				
Sine	} meridian angle,	76 49 07	{	9.9740774
Cof.				
Sine	} degrees from noon,	43 52 30	{	9.9884042
V.f.				
				9.3580017
				9.8407880
				9.4458320

{ S, lat. pl.
Cof. decl. ☉
OC = 73709 N.

{ Cof. lat. pl.
S, decl. ☉
OM = 20890 S.

CM = 52819 N

9.8934439	{ V. Si. deg. à noon	OM	{	9.4458320
9.9740774				
9.8675213	{ MG	{	{	9.3199381
9.7943083				
9.5256298	{ Cof. lat. pl.	{ S, deg. à noon	{	9.7943083
9.3199381				
9.7227902	{ GV	{	{	9.8407880
				9.6350963

{ Cof. merid. ang. CM	9.3580017 9.7227902	{ S, merid. ang. CM	9.9884042 9.7227902
{ Merid. par. lon. } D à ☉, E.	12045, E, 9.0807919	{ Merid. par. lat. } D à ☉, S.	= 51427, S. 9.7111944

Variation of PAR. LON.

{ Cof. merid. ang. MG	9.3580017 8.7657701	{ S merid. ang. MG	9.9884042 8.7657701
{ 1st PART = 1330, E.	8.1237718	{ 1st PART = 5678, S.	8.7541743
{ S, merid. ang. GV	9.9884042 9.6350963	{ Cof. merid. ang. GV	9.3580017 9.6350963
{ 2d PART = 42024, E.	9.6235005	{ 2d PART = 9842, N.	8.9930980

Total 43354, E.
Merid. par. 12045, E.

{ Par. lon. 55399, E. Horiz. par. D à ☉ = 53' 58"	9.7435019 3.5103768	{ Par. lat. = 47263, S. Horiz. par. D à ☉	9.6745213 3.5102768
{ Tr. par. lon. D à ☉ = 29 54, E. Tr. lon. D à ☉, W. = 58 34, W.	3.2537787	{ Tr. par. lat. D à ☉ = 25' 30", S. Tr. lat. D = 33, 56, N.	3.1847981
Vis. lon. D à ☉, W. = 28 40, W.		Vis. lat. D à ☉ =	8, 26, N.

Dist. VERTEX from the Disk.

Cof. lat. pl.	9.7943083	S, lat. pl.	9.8934439
Cof. decl. ☉	9.9740774	S, decl. ☉	9.5256298
Cof. deg. à noon	9.8578476		9.4190737
	9.6262333	1st PART = 26247,	
		2d PART = 42290.	
Total dist.	—	68537,	9.8359251
zds in radius, co. ar.	—	—	4.6855748
Horiz. par. D à ☉	—	—	3.5102768
Semid. ☉	—	15' 52"	2.9786369
zds to be subtracted	—	10	1.0104136
Curtate semid. ☉	—	15 42	

N. B. A late pamphlet on this subject having been published, (intituled, *A new and compendious Method of investigating the Parallactic Angle, without regard to the Nonagesimal Degree.* Addressed to the R. Hon. the E. of Macclesfield.) and afterwards inserted in the philosophical transactions; the Author of the foregoing paper thinks proper to intimate, that he had the substance of it by

him in writing, long before he saw that pamphlet. And, tho' the two methods may seem to be pretty similar, yet he thinks the operations by the foregoing rules will, upon comparison, be found to be rather more accurate, and, at the same time, much easier; as the trouble of working by the tables of fines and tangents is here, in a great measure, avoided.

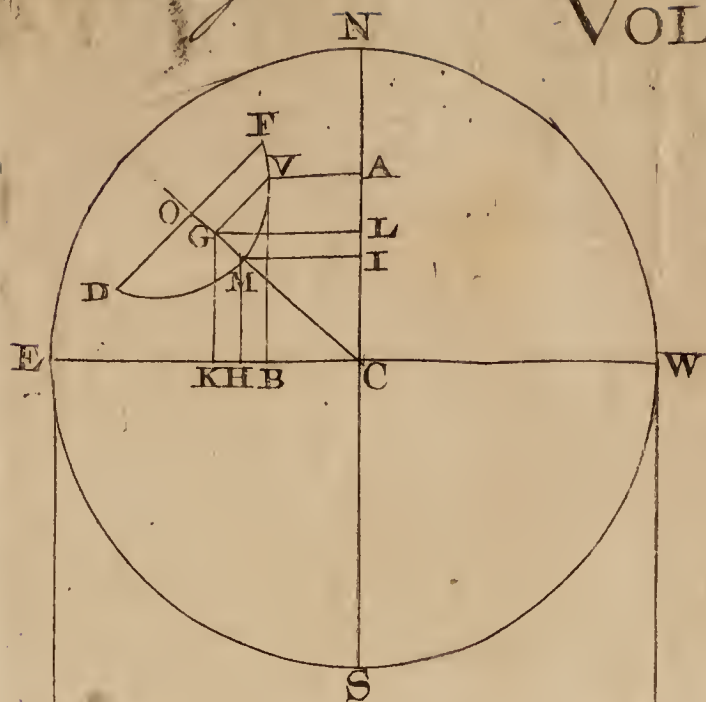
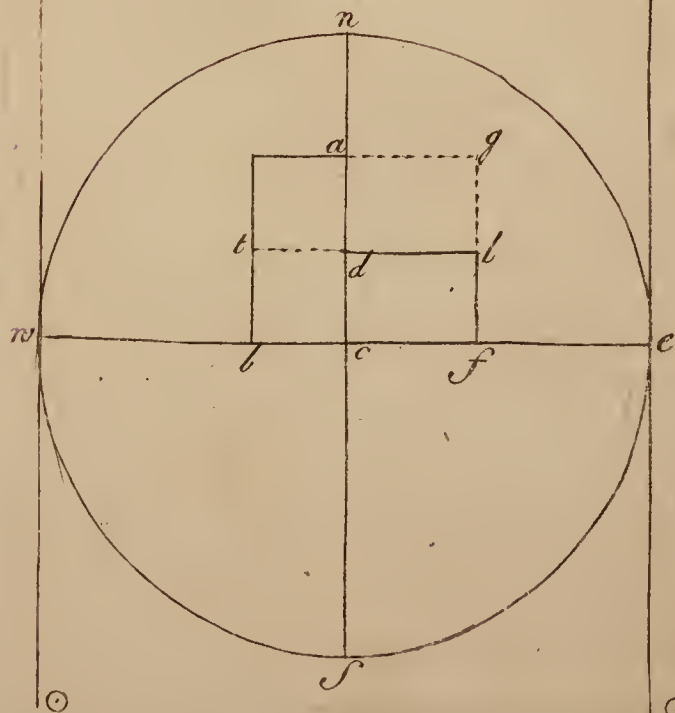
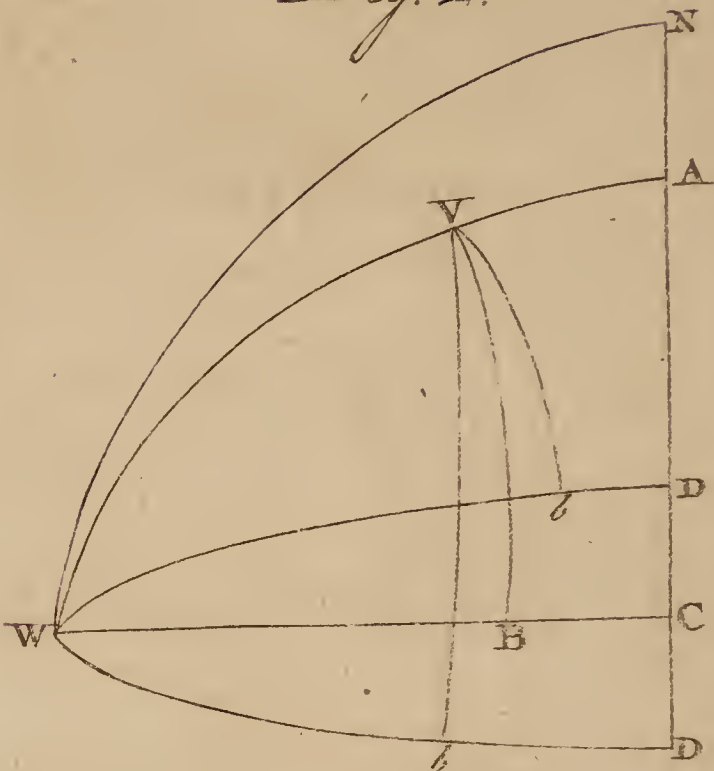
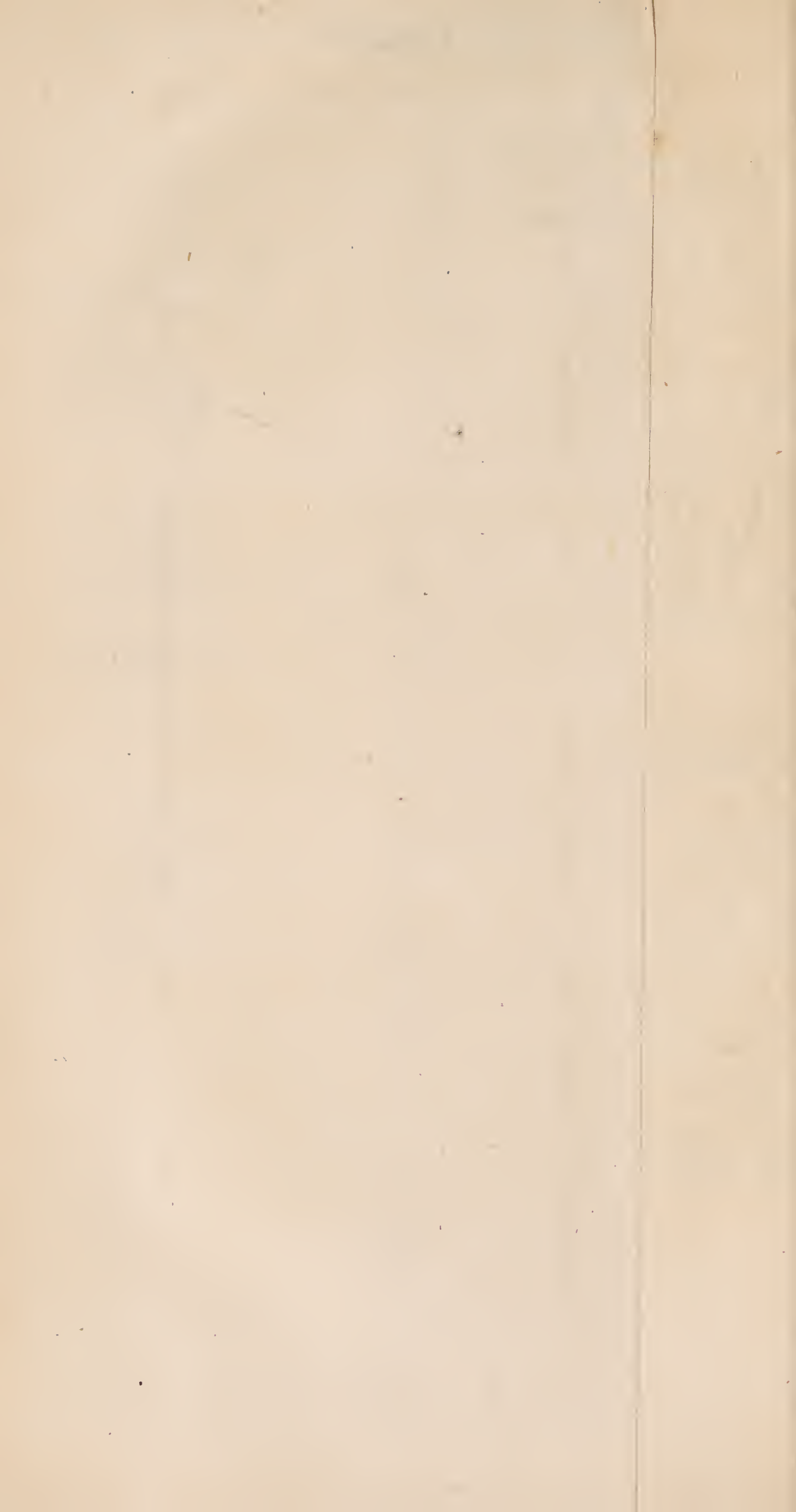


Fig. 2.





ART. VI.

*A Solution of Kepler's Problem; by MATTHEW STEWART, Professor of Mathematics in the University of Edinburgh *.*

TH^O' the problem proposed by the celebrated *Kepler* has been solved by several Mathematicians of great note: yet, as this is of considerable use in astronomy, it is hoped the following solution may be agreeable to some; as it requires a less degree of knowledge in the more difficult parts of Mathematics, than the other methods do.

P R O P. I. *Tab. V. Fig. 1.*

Let there be a straight line AB, and CD a portion of a curve wholly concave towards AB, and draw AC, BD parallel to each other, meeting the curve in C, D; let CE, a tangent

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* December 4, 1755.

gent to the curve at C , meet BD in E ; join AD , AE , BC , and let AE meet the curve in F : the triangle ABC will be greater than the sector ACF , but less than the sector ACD .

JOIN CD . Because AC , BE , are parallel, the triangles, ABC , AEC , will be equal: but the triangle AEC is greater than the sector ACF ; therefore the triangle ABC is greater than the sector ACF . Again, because AC , BD , are parallel, the triangles, ABC , ADC , are equal: but the triangle ADC is less than the sector ACD ; therefore the triangle ABC is less than the sector ACD .

P. R O P. II. *Fig. 2.*

Let there be a curve AEB , wholly concave towards the straight line AB , and let C , D , be two points in the line AB ; draw DE to any point E .

E in the curve, and draw CF parallel to DE, meeting the curve in F; let EG, a tangent to the curve at E, meet CF in G, and join DF, DG, and let DG meet the curve in H; let the point D be between the points, A, C: the sector ACE will be greater than the sector ADH, but less than the sector ADF; and the sector BCE will be greater than the sector BDF, but less than the sector BDH.

BECAUSE the triangle DEC is greater than the sector EDH, let the sector ADE be added to both; and the space ACE will be greater than the sector ADH: and, because the triangle DEC is less than the sector EDF, let the sector ADE be added to both; and the sector ACE will be less than the sector ADF.

AGAIN, because the sector ACE together with the sector BCE, is equal to the sector ADF together with the sector BDF, and the
sector

sector ACE is less than the sector ADF ; therefore the sector BCE is greater than the sector BDF : and, because the sector ACE together with the sector BCE, is equal to the sector ADH together with the sector BDH, and the sector ACE is greater than the sector ADH ; therefore the sector BCE will be less than the sector BDH.

PROP. III. PROBL. I. *Fig. 3.*

Let there be a given circle AEB, and let D be a given point within the circle ; and from D let there be drawn DE, DF, to two given points, E, F, in the circle : granting the quadrature of the circle, it is required to draw a line DG meeting the arc FE in G ; so that the sector EDG may be to the sector GDF in a given ratio, suppose that of m to n .

LET C be the center of the circle, and join CE, CF ; draw DH, DK, perpendicular to
CE,

CE, CF, meeting CE, CF in H, K ; and let EL, FM, be tangents to the circle at the points, E, F.

BECAUSE the points, D, E, F, are given, and the circle is given, the sector DEF will be given ; and, because the sector EDG is to the sector GDF in the given ratio of m to n , the sectors, EDG, GDF, will, each of them, be given in magnitude. In the tangent EL take the point L towards F ; so that, joining HL, the triangle HEL may be equal to the sector EDG : again, in the tangent FM take the point M towards E ; so that, joining KM, the triangle KFM will be equal to the sector FDG : it is evident, the points, L, M, will be given. Join DL, DM, meeting the circle in N, O ; the point G will fall between the points, N, O. For, because EL, FM, are tangents to the circle at E, F, the angles HEL, KFM, will be right ; and therefore DH, EL, will be parallel : likewise DK, FM, will be parallel ; therefore the triangles, DEL, HEL, will be equal, and likewise the triangles, DFM, KFM, will be equal : and therefore the triangle DEL will be equal to the sector DEG, and the triangle DFM equal

to the sector DFG; therefore the sector DEG will be greater than the sector DEN, and the sector DFG greater than the sector DFO; therefore the line DG will fall between the lines, DN, DO; therefore the point G will fall between the points, N, O.

AGAIN, Because the points, L, M are given, and the point D likewise given, the sectors, DEN, DFO, will be given; and, because the sectors, DEG, DFG, are given, the sectors, DNG, DOG, will be given; and therefore the sector NDG will be to the sector GDO in a given ratio, suppose that of p to q . The problem, therefore, will be reduced to this again, To divide the sector NDO by the line DG, so that the sector NDG may be to the sector GDO in the given ratio of p to q . And thus, repeating the operation, the point G will be found within a very narrow limit.

N. B. THE limit ON might have been found by taking in the tangents, EL, FM, the point L towards F, and the point M towards E; so that, joining DL, DM, meeting the circle in N, O, the triangle DEL would be equal to the sector EDG, and the triangle DFM

DFM equal to the sector DFG: but the other way points out the method of calculation.

PROP. IV. PROBL. 2. *Fig. 4.*

Let there be a semicircle whose diameter is AB and center C, and let D be a point in the diameter: granting the quadrature of the circle, it is required to draw a line DE meeting the circle in E, so that the semicircle may be to the sector BDE in a given ratio, suppose that of p to q .

SUPPOSE the problem solved. Let the semicircle be to the sector BCF as p is to q ; join DF; draw CG parallel to DF, meeting FG, a tangent to the circle at F, in G, and join DG meeting the circle in H; let CG meet the circle in K, and join DK, FK, CH.

BECAUSE the semicircle is to the sector BDE as p to q , that is, as the semicircle to the
sector

sector BCF, the sector BDE will be equal to the sector BCF: but the sector BCF is [*Prop. 2.*] greater than the sector BDK, but less than the sector BDH; therefore the sector BDE is greater than the sector BDK, and less than the sector BDH; therefore the line DE falls between the lines, DH, DK; and therefore the point E falls between the points, H, K. Again, because the sector BDE is equal to the sector BCF, and the sector BCK is common to both, the space KCDEK will be equal to the sector KCF; and, because the triangles, KCD, KCF, are equal, (because KC, DF, are parallel), therefore the sector KDE is equal to the space KEFK; and, because the triangles, GDK, GFK, are equal, taking the common space GHK from both, the sector KDH will be equal to the space KEFK together with the space GFHG; but the sector KDE is equal to the space KEFK, therefore the sector EDH is equal to the space GFHG.

BECAUSE the semicircle is to the sector BCF as p to q , therefore the angle BCF is given, and therefore the angle FCD is likewise given: and, in the triangle FCD, because the sides, CF, CD, are given, and likewise

ways the angle FCD , the angles CDF , CFD will be given; and therefore the angles BCK , KCF will be given; therefore the arcs BK , KF are given: because FG is a tangent to the circle at the point F , the angle CFG will be a right angle: and because the angle FCG is given, and likewise the side CF given, the sides CG , CF will be given: in the triangle GCD , because the sides GC , CD are given, and the angle GCD given, the angles CGD , CDG will be given: and in the triangle GCH , because the sides GC , CH and the angle CGH is given, the angle GCH will be given; and therefore the arc HK will be given: and because the arc FK is given, therefore the arc FH is given. Again, because the arc FK is given, the sector FCK will be given, and likewise the triangle FCK given, therefore the space $KEFK$ will be given; and because the triangles, GFC , GHC , are given, the space $GFCHG$ will be given; and because the arc FH is given, the sector FCH will be given; therefore the space $GFHG$ will be given.

From this, the following construction may be deduced.

CONSTRUCTION.

In the semicircle take the point F , and let the arc AFB be to the arc BF as p is to q ; join CF , DF , and draw CG parallel to DF , meeting FG , a tangent to the circle, at F in G ; and the circle in K ; join DG meeting the circle in H , and join FK , DK ; draw the line DE [*Prop. 3.*] meeting the circle in E , so that the sector KDE may be to the sector EDH as the space $KEFK$ to the space $GFHG$: the semicircle will be to the sector BDE as p is to q .

BECAUSE the sector KDE is to the sector EDH , as the space $KEFK$ to the space $GFHG$; the sector EDH will be to the sector KDH , as the space $KEFK$ to the sum of the spaces $KEFK$, $GFHG$: but, because
the

the triangles, GDK, GFK are equal, and the space GHK common to both, the sector KDH will be equal to the sum of the spaces KEFK, GFHG ; therefore the sector KDE is equal to the space KEFK. And, because the triangle KDC is equal to the triangle KFC, the space KCDE will be equal to the sector KCF; therefore (adding the sector BCK to both) the sector BDE will be equal to the sector BCF. Again, because the arc AFB is to the arc BF as p is to q , the semicircle will be to the sector BCF as p is to q ; therefore the semicircle will be to the sector BDE as p is to q . Q. E. D.

BUT, as this would require a good deal of trigonometrical calculation, the following method may be used; which will give the point sought very nearly, when this problem is of use in the planetary system.

PROP. V. PROBL. 3. *Fig. 5.*

Let there be a semicircle whose diameter is AB and centre C, and let D be a point in the diameter not
very

very excentric ; granting the quadrature of the circle, it is required to draw a line, DE , meeting the circle in E , so that the semicircle may be to the sector BDE in a given ratio, suppose that of p to q .

SUPPOSE the problem solved ; and let the semicircle be to the sector BCF as p is to q ; join DF , and draw CG parallel to DF meeting FG a tangent to the circle at F in G ; join DG , and let CG , DG meet the circle in H , K : join DH , FH ; draw CM parallel to DH meeting HM a tangent to the circle at H in M ; draw DL perpendicular to CH meeting CH in L ; join LE , and let FN perpendicular to CH meet CH in N .

BECAUSE the semicircle is to the sector BDE as p is to q , that is, as the semicircle to the sector BCF , the sector BDE will be equal to the sector BCF : and because the sector BCH is common to both, the space $HCDE$ will be equal to the sector HCF ; but because CH , DF are parallel, the triangle DCH is equal to the triangle FCH ; there-
fore

fore the sector EDH will be equal to the space contained by the arc HF and the chord FH. Again, because the sector BDE is equal to the sector BCF, and the sector BCF is [*Prop. 2.*] greater than the sector BDH, and less than the sector BDK, the sector BDE will be greater than the sector BDH, and less than the sector BDK: therefore the point E is in the arc HK between the points H, K. [Let the arc HK be called the limiting arc]. Because the point D is not very excentric, the limiting arc will be little. In the orbit of *Mercury*, the most excentric of all the planets, the limiting arc will be less than sixteen minutes in that part of the orbit where it is greatest; therefore EH may be considered as coinciding with the tangent to the circle at the point H; and therefore the triangle ELH will be equal to the sector EDH, that is, equal to the space contained by the arc FH and the chord FH; therefore the rectangle LHE will be double of the space contained by the arc FH and the chord FH. Because the rectangle contained by CH and the arc FH is double of the sector CFH, and the rectangle contained by CH, FN is double of the triangle CFH; the
rectangle

rectangle contained by CH and the excess of the arc FH above FN will be double of the space contained by the arc FH and the chord FH; therefore the rectangle LHE will be equal to the rectangle contained by CH and the excess of the arc FH above FN; therefore LH will be to HC as the excess of the arc FH above FN to HE: but because the triangles DLH, CHM are similar, LH will be to HC as DL or FN to HM; therefore FN will be to HM as the excess of the arc FH above FN to HE.

From this, the following construction may be deduced.

CONSTRUCTION.

Let the semicircle be to the sector BCF as p is to q ; join DF, draw CH parallel to DF meeting the circle in H; join DH, draw CM parallel to DH meeting HM a tangent to the circle at H in M; and let FN perpendicular to CH meet CH in N; in the tangent
HM

HM take HE towards F, so that FN may be to HM as the excess of the arc FH above FN to HE; join DE: the semicircle will be to the sector BDE as p is to q .

LET DL, perpendicular to CH, meet CH in L; join FH, LE. Let CH meet FG, a tangent to the circle at F, in G; join DG meeting the circle in K.

BECAUSE the triangles DLH, CHM are similar, LH will be to HC as DL or FN to HM; that is, as the excess of the arc FH above FN to HE: therefore the rectangle LHE will be equal to the rectangle contained by HC and the excess of the arc HF above FN: but, because the rectangle contained by CH and the arc HF is double of the sector HCF, and the rectangle contained by CH, FN is double of the triangle CFH; therefore the rectangle contained by CH and the excess of the arc FH above FN will be double of the space contained by the arc FH and the chord FH; therefore the rectangle LHE will be double of the space contained by the arc FH and the chord FH;
and

and therefore the triangle LHE will be equal to the space contained by the arc FH and the chord FH: but, because DL, HE are parallel, the triangles DHE, LHE are equal; therefore the triangle DHE is equal to the space contained by the arc FH and the chord FH: and, because DF, CH are parallel, the triangles CDH, CFH are equal; therefore the space HCDE will be equal to the sector CFH. Let the sector BCH be added to both, and the sector BDE will be equal to the sector BCF: but, because the sector BCF is greater than the sector BDH and less than the sector BDK, therefore the sector BDE is greater than the sector BDH and less than the sector BDK, and therefore DE falls between DH, DK: and, because the point D is not very excentric, the limiting arc HK will be little; therefore the tangent HE may be considered as coinciding with the arc HK: because the sector BDE is equal to the sector BCF, the semicircle will be to the sector BDE as the semicircle to the sector BCF: but the semicircle is to the sector BCF as p is to q ; therefore the semicircle is to the sector BDE as p is to q .
 Q. E. D.

Kepler

Kepler first of all discovered that the planets revolved in ellipses round the sun placed in one of the *foci*, and that they described equal areas in equal times round the sun. Let the semi-ellipse, [*Fig. 6.*] whose greater *axis* is *AP*, *focus* *S*, and centre *C*, represent half the orbit of a planet round the sun in *S*; and suppose the planet at the point *K* in its orbit; join *SK*: half the periodic time of the planet round the sun, is to the time the planet moves from *A* to *P*, as the area of the semi-ellipse to the area *ASK*; and therefore to find the place of the planet at any given time, it is necessary to find the position of the right line *SK*, which shall cut off the area *ASK* proportional to the time, that is, To draw the line *SK* so that the area of the semi-ellipse may be to the area *ASK*, as half the periodic time of the planet round the sun to the given time.

FROM *K* let fall *KH* perpendicular to *AP*, meeting the semicircle described upon *AP* in *G*, and join *SG*: it is evident, from the nature of the ellipse and circle, that the semicircle is to the sector *ASG* as the semi-ellipse to the sector *ASK*; therefore the semicircle is to the sector *ASG* as half the periodic time of

the planet round the sun to the time the planet moves from A to K: the problem therefore is reduced to this, To draw the line SG meeting the semicircle in G, so that the semicircle may be to the sector ASG as half the periodic time of the planet round the sun to the given time. In the semicircle, take the arc AB, so that the arc ABP may be to the arc AB as half the periodic time of the planet round the sun to the given time. Join CB; the semicircle therefore will be to the sector ACB as half the periodic time of the planet round the sun to the given time, that is, as the semicircle to the sector ASG; therefore the sectors ACB, ASG, are equal; join CG.

The angle ACB is called by *Kepler* the mean anomaly, the angle ACG the anomaly of the excentric, and the angle ASK the co-equate or true anomaly. The problem therefore is reduced to this: The mean anomaly of a planet being given, to find the anomaly of the excentric and the coequate anomaly.

PROP.

PROP. VI. PROBL. IV. *Fig. 6.*

Let AP be the greater axis of a planet's orbit, S the focus the place of the sun, A the aphelion, P the perihelion; upon AP let the femi-circle ABP be described; let C be the center, and let the angle ACB be the mean anomaly of the planet at any given time: it is required to find the anomaly of the excentric.

JOIN SB , and draw CD parallel to SB meeting the circle in D : join SD in BD : take the arc DG , so that the sine of the angle BCD may be to the tangent of the angle CDS , as the excess of the arc BD above its sine to the arc DG ; and join CG : the angle ACG will be nearly the anomaly of the excentric.

JOIN SG ; draw BE perpendicular to CD meeting CD in E , and draw CF parallel to SD meeting DF a tangent to the circle at D in F .

BECAUSE

BECAUSE BE is the sine of the angle BCD, and DF the tangent of the angle DCF, that is, of the angle SDC; BE will be to DF as the excess of the arc BD above BE to the arc DG; therefore [*Prop. 5.*] the sector ASG is equal to the sector ACB; therefore the angle ACG is the anomaly of the excentric.

THE computation is as follows: In the triangle BCS, as the sum of the sides BC, CS, is to the difference of the sides BC, CS, so is the tangent of half the angle ACB to the tangent of half the difference of the angles, CSB, CBS; therefore the angles, CSB, CBS will be given, that is, the angles, ACD, DCB, will be given. Again, in the triangle CSD, the sum of the sides, CD, CS, is to the difference of the sides CD, CS, as the tangent of half the angle ACD to the tangent of half the difference of the angles, CSD, CDS; therefore the angle CDS will be given. Again, because as the sine of the angle BCD is to the tangent of the angle CDS, so is the excess of the arc BD above its sine to the arc GD; say as the radius is to the sine of the angle BCD, so is $57^{\circ}.2957795$ &c. the number of degrees in an angle subtended
by

by an arc equal to the radius, to the number of degrees in an angle subtended by an arc equal to the sine of the angle BCD; [let this angle be called A]. Again, as the sine of the angle BCD to the tangent of the angle CDS, so is the excess of the angle BCD above the angle A, to the angle GCD. Therefore the angle ACG, the anomaly of the excentric, will be given.

E X A M P L E I.

IN the orbit of *Mercury*, the mean distance is to the excentricity as 100000 to 20589. Suppose the mean anomaly from the aphelion to be 60° , it is required to find the anomaly of the excentric. In the triangle BCS, as 120589, the sum of BC, CS, is to 79411 the difference of the sides BC, CS, so is the tangent of 30° , half the sum of the angles, CSB, CBS, to the tangent of half the difference of the angles CSB, CBS.

The log. tang. of 30° is	9.7614394	
The log. of 79411 is	4.8998807	
	<hr/>	
The sum is	14.6613201	
The log. of 120589 is	5.0813077	
	<hr/>	
The difference	9.5800124	is the
		log.

log. tang. of $20^{\circ} 49'.00894$ half the difference of the angles CSB, CBS ; therefore the angle CSB or ACD is $50^{\circ} 49'.00894$, and the angle CBS or BCD is $9^{\circ} 10'.99106$. Again, in the triangle DCS, as 120589 the sum of DC, CS, is to 79411 the difference of DC, CS, so is the tangent of $25^{\circ} 24'.50447$ half the sum of the angles CSD, CDS, to the tangent of half the difference of the angles CSD, CDS.

The log. tang. of $25^{\circ} 24'.50447$ is 9.6767070

The log. of 79411 is 4.8998807

The sum is 14.5765877

The log. of 120589 is 5.0813077

The difference 9.4952800

is the log. tang. of $17^{\circ} 22'.21093$ half the difference of the angles CSD, CDS ; therefore the angle CDS is $8^{\circ} 2'.29354$. Again, as the radius is to the sine of $9^{\circ} 10'.99106$, so is $57^{\circ}.2957795$ &c. the number of degrees in an angle subtended by an arc equal to the radius, to the number of degrees and minutes in an angle subtended by an arc equal to the sine of $9^{\circ} 10'.99106$.

The

The log. of 57.2957795 is 1.7581226

The log. fine of $9^{\circ} 10'.99106$ is 9.2030097

The sum is 10.9611323

The log. of rad. is 10.0000000

The difference 0.9611323

is the log. of $9^{\circ} 8'.6349999$, the number of degrees and minutes contained in an angle subtended by an arc equal to the fine of $9^{\circ} 10'.99106$. The angle A therefore is $9^{\circ} 8'.6349999$; the excess therefore of the angle BCD above the angle A is $2'.35606$. Again, as the fine of $9^{\circ} 10'.99106$ is to the tangent of $8^{\circ} 2'.29354$, so is the angle $2'.35606$ to the angle DCG.

The log. of $2'.35606$ is 0.3721863

The log. tang. of $8^{\circ} 2'.29354$ is 9.1498998

The sum is 9.5220861

The log. fine of $9^{\circ} 10'.99106$ is 9.2030097

The difference 0.3190964

is the log. of $2'.084953$ the angle DCG, the angle ACG the anomaly of the excentric is $50^{\circ} 51'.093893$, that is $50^{\circ} 51' 5''.63358$.

LET

LET SM, perpendicular to CG, meet CG in M: It is evident, because the sector ACG is common to both the sectors ASG, ACB, that, if the sector ASG be equal to the sector ACB, the triangle SCG will be equal to the sector BCG; and therefore the line SM will be equal to the arc BG: if the sector ASG be greater than the sector ACB, the triangle SCG will be greater than the sector BCG; and therefore the line SM will be greater than the arc BG: and, if the sector ASG be less than the sector ACB, the triangle SCG will be less than the sector BCG; and therefore the line SM will be less than the arc BG: that is, if the arc AG be greater, equal, or less, than the true anomaly of the excentric, the line SM will be greater, equal, or less, than the arc BG; and therefore, the less the difference is between the line SM and the arc BG, the less will the difference be between the arc AG and the anomaly of the excentric.

BECAUSE the triangles GCH, CSM, are similar, CG is to GH as CS to SM; that is, the radius is to the sine of $50^{\circ} 51'.093893$ as 20589 to CM.

The

The log. of 20589 is 4.3136353

The log. sine of $50^{\circ} 51'.093797$ is 9.8895890

The sum is 14.2032243

The log. of rad. is 10.0000000

The difference 4.2032243

is the log. of SM. Again, as CA to SM, so is $57^{\circ}.2957795$, &c. the number of degrees in an angle subtended by an arc equal to CA, to the number of degrees in an angle subtended by an arc equal to SM.

The log. of $57^{\circ}.2957795$, &c. is 1.7581226

The log. of SM is 4.2032243

The sum is 5.9613469

The log. of CA 5.0000000

The difference 0.9613469

is the log. of $9^{\circ} 8'.90625$ the number of degrees and minutes in an angle subtended by an arc equal to SM: but, because the arc AB is 60° , and the arc AG is $50^{\circ} 51'.093797$, the arc BG will be $9^{\circ} 8'.906203$; the difference therefore between SM and the arc BG

is 0'.000047 ; the difference therefore is less than the 354th part of a second.

Mr. *Machin*, in his solution of this problem in the Philosophical Transactions, Number 447. makes the anomaly of the excentric to be $129^{\circ}.14846$, when the mean anomaly reckoned from the perihelion is 120° ; and therefore, if the mean anomaly reckoned from the aphelion be 60° , the anomaly of the excentric will be $50^{\circ}.85154$, that is $50^{\circ} 51'.0924$.

IN order to determine the difference between SH and the arc BG according to this computation,

The log. of 20589 is	4.3136353
The log. sine of $50^{\circ} 51'.0924$ is	9.8895888
	<hr/>
The sum is	14.2032241
The log. of rad. is	10.0000000
	<hr/>
The difference	4.2032241

is the log. of SM. Again, as CA to SM, so is $57^{\circ}.2957795$, &c. the number of degrees in an angle subtended by an arc equal to CA, to the number of degrees in an angle subtended by an arc equal to SM.

The

The log. of $57^{\circ}.2957795$, &c. is 1.7581226

The log. of SM is 4.2032241

The sum is 5.9613467

The log. of CA is 5.0000000

The difference 0.9613467

is the log. of $9^{\circ} 8'.905998$, the number of degrees and minutes in an angle subtended by an arc equal to SM: but, because the arc AB is 60° , and the arc AG is $50^{\circ} 51'.0924$, the arc BG will be $9^{\circ} 8'.9076$; the difference therefore between SM and the arc BG is $0'.001602$, very nearly one tenth of a second, and is more than thirty four times greater than the former difference.

BECAUSE AS is equal to the sum of BC, CS, and PS equal to the difference of BC, CS; it is evident, that, if, from the log. tang. of half the angle ACB, the difference of the logarithms of AS, SP be subtracted, the remainder will be the log. tang. of an angle, which, if added to half the angle ACB, will give the angle ASB or ACD; and, if subtracted from half the angle ACB, will give the angle CBS or BCD.

EX-

E X A M P L E II.

IN the orbit of *Mars*, the mean distance is to the excentricity as 152369 to 14100. Supposing the mean anomaly to be 1° , it is required to find the anomaly of the excentric,

The log. tang. of $30'$, half the angle ACB, is 7.9408584

The difference of the logs. of AS, SP is 0.0806086

The difference 7.8602498

is the log. tang. of $24'.9196814$, half the difference of the angles CSB, CBS; therefore, the angle CSB, that is the angle ACD, is $54'.9196814$; and the angle CBS, that is the angle BCD, is 5.0803186 . Again, to determine the angle CDS,

The log. tang. of half the angle ACD is 7.8958249

The difference of the logs. of AS, SP is 0.0806086

The difference 7.8152163

is the log. tang. of $22'.4641426$, half the difference of the angles CSD, CDS; therefore

fore the angle CDS, that is the angle DCF, is 4'.9956981.

AGAIN, as the radius is to the sine of 5'.0803186, so is $57^{\circ} 29' 77'' 95$, &c. the number of degrees in an angle subtended by an arc equal to the radius, to the number of degrees and minutes in an angle subtended by an arc equal to the sine of 5'.0803186.

The log. of $57^{\circ} 29' 77'' 95$ is 1.7581226

The log. sine of 5'.0803186 is 7.1633313

The sum is 8.9214539

The log. of radius is 10.0000000

The difference —2.9214539

is the log. of 5'.0073162, the number of minutes contained in the angle A; the excess therefore of the angle BCD, above the angle A, is 0'.0930024. Again, as the sine of the angle BCD is to the tangent of the angle DCF, so is 0'.0930024 to the angle DCG.

The log. of 0'.0930024 is —2.9684941

The log. tang. of the angle DCF is 7.1622795

The sum is 6.1307736

The log. sine of the angle BCD is 7.1633313

The difference 2.9674423

is

is the log. of $0'.0927774$; the angle DCG therefore is $0'.0927774$; therefore the angle ACG, the anomaly of the excentric, is $55'.0124588$.

E X A M P L E III.

SUPPOSING the mean anomaly in the same orbit to be 45° , it is required to find the anomaly of the excentric.

The log. tang. of $22^\circ 30'$ is 9.6172243

The diff. of the logs. of AS, SP is 0.0806085

The difference 9.5366158

is the log. tang. of $18^\circ 59'.1327325$, half the difference of the angles CSB, CBS; the angle CSB, that is the angle ACD, therefore is $41^\circ 29'.1327325$; and the angle CBS or BCD is $3^\circ 30'.8672675$.

The log. tang. of half the angle

ACD is 9.5783203

The diff. of the logs. of AS, SP is 0.0806085

The difference 9.4977118

is the log. tang. of $17^\circ 27'.7082672$, half the difference of the angles CSD, CDS; the angle CDS or DCF, therefore, is $3^\circ 16'.858099$.

$3^{\circ} 16'.858099$. Again, as radius is to the fine of the angle BCD, so is $57^{\circ}.2957795$, &c. to the angle A.

The log. of $57^{\circ}.2957795$, &c. is 1.7581226

The log. fine of the angle BCD is 8.7874623

The sum is 10.5455849

The log. of rad. is 10.0000000

The difference 0.5455849

is the log. of $3^{\circ}.5122459$; the angle A, therefore, is $3^{\circ} 30'.734754$; the excess of the angle BCD above the angle A is $0'.1325135$. Again, as the fine of the angle BCD is to the tangent of the angle SDC or DCF, so is $0'.1325135$ to the angle DCG.

The log. of $0'.1325135$ is -1.1222601

The log. tang. of the angle SDC is 8.7583537

The sum is 7.8806138

The log. fine of the angle BCD is 8.7874623

The difference -1.0931515

is the log. of $0'.1239228$; the angle DCG, therefore, is $0'.1239228$; therefore the angle ACG, the anomaly of the excentric, is $41^{\circ} 29'.2566553$.

EX-

EXAMPLE IV.

AGAIN, in the same orbit supposing the mean anomaly to be 100° degrees, it is required to find the anomaly of the excentric.

The log. tang. of 50° half the angle ACB is 10.0761865

The difference of the logs. of AS, SP is 0.0806086

The difference 9.9955779

is the log. tang. of $44^\circ 42'.4982192$, half the difference of the angles CSB, CBS; therefore the angle CSB, that is the angle ACD, is $94^\circ 42'.4982192$; and the angle CBS, that is the angle BCD, is $5^\circ 17'.5017808$. Again, to determine the angle CDS,

The log. tang. of half the angle ACD is 10.0557285

The difference of the logs. of AS, SP is 0.0806086

The difference 9.9751199

is the log. tang. of $43^\circ 21'.5819834$, half the difference of the angles CSD, CDS; therefore the angle CDS, that is the angle DCF, is $3^\circ 59'.6671212$.

AGAIN,

AGAIN, as the radius is to the sine of $5^{\circ} 17'.5017808$ the angle BCD, so is $57^{\circ}.2997795$ to the angle A.

The log. of $57^{\circ}.2957795$ is 1.7581226

The log. sine of $5^{\circ} 17'.5017808$ is 8.9642381

The sum is 10.7223607

The log. of radius is 10.0000000

The difference 0.7223607

is the log. of $5^{\circ} 16'.600752$ the number of degrees and minutes contained in the angle A; the excess therefore of the angle BCD above the angle A is $0'.9010288$. Again, as the sine of the angle BCD is to the tangent of the angle DCF, so is $0'.9010288$ to the angle DCG.

The log. of $0'.9010288$ is —1.9547387

The log. tang. of the angle DCF is 8.8440431

The sum is 8.7987818

The log. sine of the angle BCD is 8.9642381

The difference —1.8345437

is the log. of $0'.6831934$; the angle DCG therefore is $0'.6831934$; therefore the angle ACG the anomaly of the excentric is $94^{\circ} 43'.1814126$.

THE three last examples are taken from Dr. *Keil's* astronomical Lectures, Lecture 23d. and the numbers agree very nearly with his.

IN the orbits of *Mercury* and *Mars*, if the excess of the angle BCD above the angle A be added to the angle ACD, the sum will be nearly the anomaly of the excentric reckoned from the aphelion.

IN orbits of small excentricity, the angle ACD is nearly the anomaly of the excentric; therefore the following rule will give the anomaly of the excentric very nearly.

FROM the logarithmic tangent of half the mean anomaly, subtract the difference of the logarithms of the aphelion and perihelion distances; the remainder is the logarithmic tangent of an angle, which call B: to the angle B, add half the mean anomaly; the sum will give very nearly the anomaly of the excentric.

E X A M P L E V.

IN the earth's orbit, the mean distance is to the excentricity as 100000 to 1691. Suppose the mean anomaly from the aphelion to be 30° , it is required to find the anomaly of the excentric.

The

The log. tang. of 15° is 9.4280525

The diff. of the logs. of AS, SP is 0.0146892

The difference 9.4133633

is the log. tang. of $14^\circ 31'.3670421$, half the difference of the angles CSB, CBS; therefore the angle CSB, that is the angle ACD, is $29^\circ 31'.367042$; therefore the anomaly of the excentric is nearly $29^\circ 31'.3670421$, that is $29^\circ 31'.22''.022526$, which agrees very nearly with Dr. *Keil's* numbers in the foresaid Lecture.

EXAMPLE VI.

AGAIN, in the earth's orbit, suppose the mean anomaly to be 60° , it is required to find the anomaly of the excentric.

The log. tang. of 30° is 9.7614394

The diff. of the logs. of AS, SP is 0.0146892

The difference 9.7467502

is the log. tang. of $29^\circ 10'.081873$, half the difference of the angles CSB, CBS; therefore the angle CSB, that is the angle ACD, is $59^\circ 10'.081873$; therefore the anomaly of the excentric is nearly $59^\circ 10'.081873$.

EX-

E X A M P L E VII.

IN the orbit of *Venus*, the mean distance is to the excentricity as 10000000 to 69855. Suppose the mean anomaly reckoned from the aphelion to be 60° , it is required to find the anomaly of the excentric.

The log. tang. of 30° is 9.7614394

The diff. of the logs. of AS, SP is 0.0060677

The difference 9.7553717

is the log. tang. of $29^\circ 39'.2739959$, half the difference of the angles CSC, CBS ; therefore the angle CSB, that is the angle ACD, is $59^\circ 39'.2739959$; therefore the anomaly of the excentric is nearly $59^\circ 39'.2739959$, that is $59^\circ 39'.16''.439754$.

IF the mean anomaly reckoned from the perihelion be 120° , the anomaly of the excentric would be nearly $120^\circ 20'.43''.560246$, which agrees very nearly with Mr. *Machin's* numbers in the forecited transaction.

THE anomaly of the excentric being found, the coequate or true anomaly will be found by the resolution of the triangle GCS : Thus, from the log. tang. of half the anomaly of the excentric, subtract the difference of the
logs.

logs. of the aphelion and perihelion distances, the remainder will be the log. tang. of an angle; to this angle add half the anomaly of the excentric; let the sum be called the angle C; to the log. tang. of the angle C, add half the sum of the logs. of the aphelion and perihelion distances; from this sum, subtract the log. of the mean distance; the remainder will be the log. tang. of the coequate or true anomaly.

LET CL be the lesser axis of the planet's orbit. Because, from the nature of the ellipse, the square of CL is equal to the rectangle ASP, the log. of CL will be equal to half the sum of the logs. of AS, SP: and, because the tangent of the angle KSH is to the tangent of the angle GSH as HK to HG; that is, from the nature of the ellipse, as LC to CA; therefore, if to the log. tang. of the angle GSH, the log. of CL be added, and from the sum the log. of AC be subtracted, the remainder will be the log. tang. of the angle KSH.

AGAIN, the sine of the true anomaly is to the sine of the anomaly of the excentric, as the lesser axis of the orbit to the distance of the planet from the sun,

BECAUSE

BECAUSE CL is to HK as CG to GH , that is, as the radius to the sine of the anomaly of the excentric, and HK is to KS as the sine of the true anomaly to the radius; therefore CL is to KS as the sine of the true anomaly to the anomaly of the excentric.

THE place of a planet in an elliptic orbit [granting the quadrature of the ellipse] may be found at any given time within a small limit, by the following theorem.

THEOREM. FIG. 7.

Let the ellipse, whose greater axis is AP , foci S , K , and center C , represent the orbit of a planet round the sun at S ; and, supposing the periodic time of the planet round the sun to be known, and likewise the time the planet passed thro' the aphelion A : As the periodic time of the planet round the sun, is to the time elapsed since the planet passed thro' the point

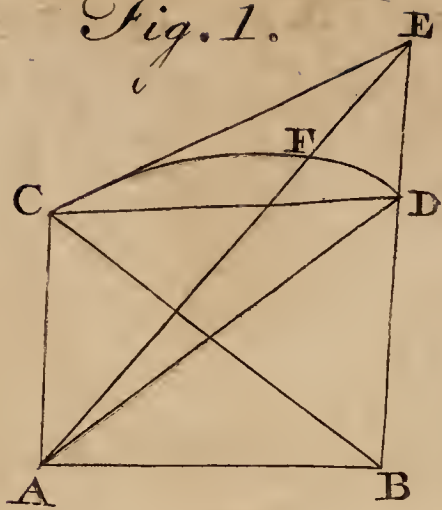
point A, so let the area of the ellipse be to the sector ACB; join SB, and draw CD parallel to SB on the same side of AP that SB is; and let CD be equal to CA; join SD; let CD, SD, meet the ellipse in E, F: the true place of the planet is between the points E, F; that is, the planet is passed the point E, but not come to the point F.

LET G be the place of the planet; join SG, and join BD meeting AP in H, and join KB; draw SL parallel to KB meeting BD in L. Because CD is parallel to SB, CD will be to SB as CH to HS; therefore, twice CD will be to SB as twice CH to HS. But, because twice CD is equal to AP, that is, equal to KB together with BS, and twice CH is equal to KH together with HS; therefore KB together with BS will be to BS as KH together with HS is to HS; and therefore KB will be to BS as KH to HS, that is, as KB to SL; therefore BS, SL are equal: therefore the angle SBL is equal to the angle SLB,

SLB, that is, equal to the angle KBD ; and therefore, from a known property of the ellipse, BD is a tangent to the ellipse at the point B ; and therefore [*Prop.* 2.] the sector ACB is greater than the sector ASE, and less than the sector ASF : but, because G is the place of the planet, the area of the ellipse will be to the sector ASG as the periodic time of the planet round the sun is to the time elapsed since the planet passed thro' the point A, that is, as the area of the ellipse to the sector ACB ; therefore the sector ASG is equal to the sector ACB ; and therefore the sector ASG is greater than the sector ASE, and less than the sector ASF ; therefore the line SG falls between the lines SE, SF ; and therefore G, the place of the planet, is between the points E, F ; therefore the planet has passed the point E, but not come to the point F.

ART.

Fig. 1.



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Fig. 2.

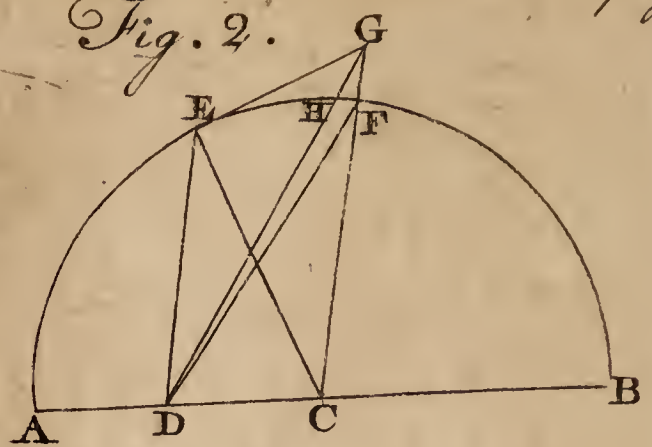


Fig. 3.

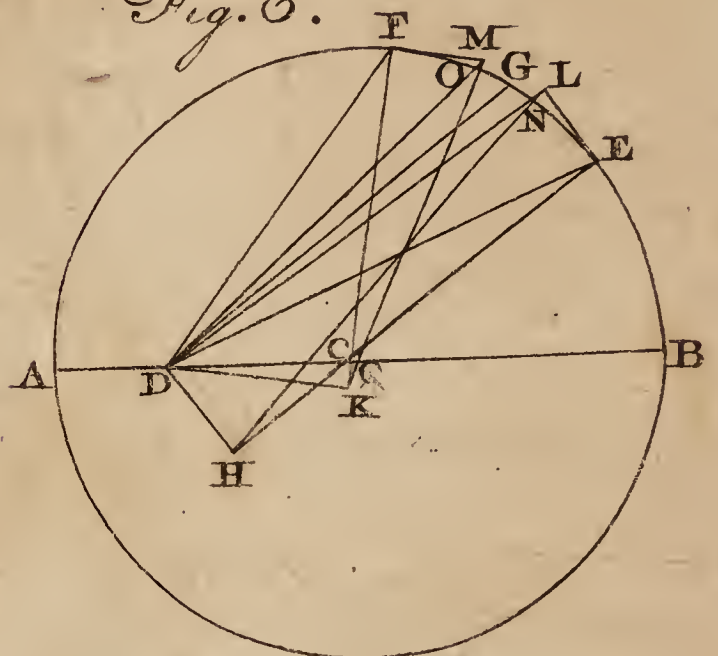


Fig. 4.

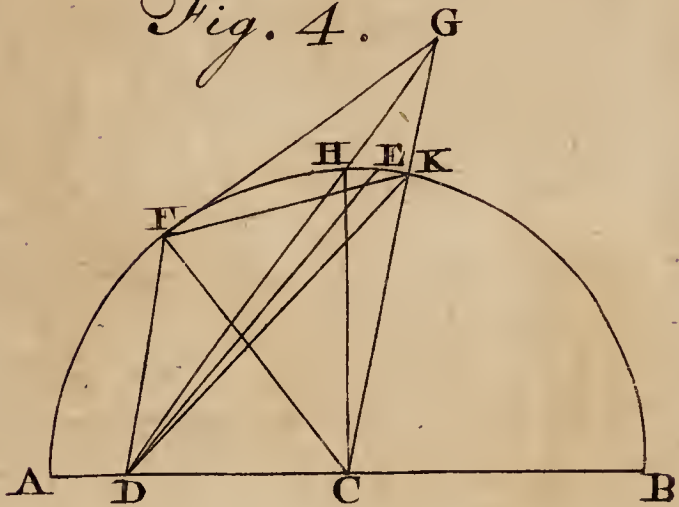


Fig. 5.

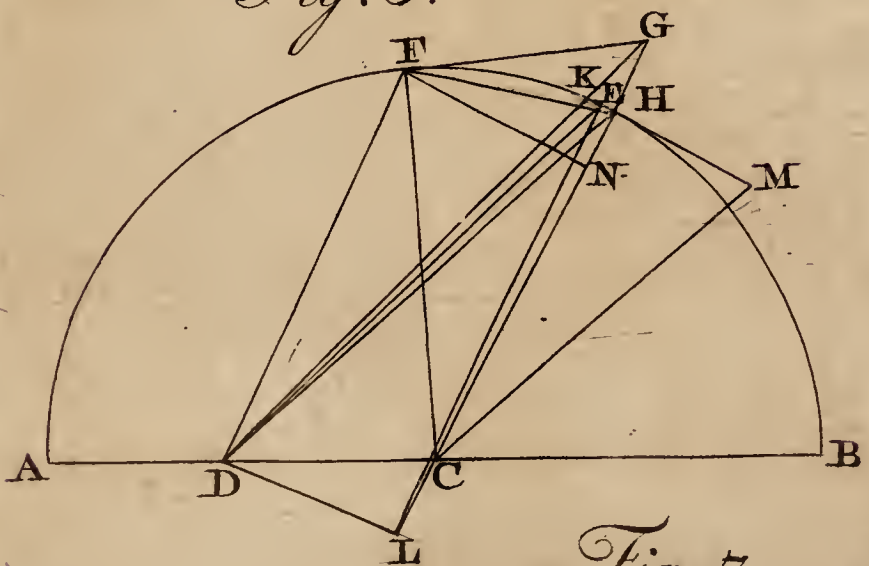


Fig. 6.

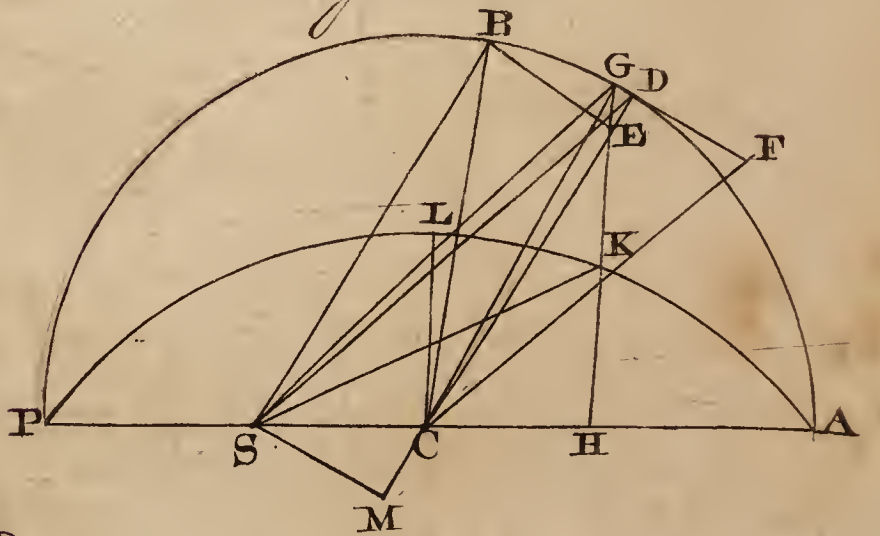
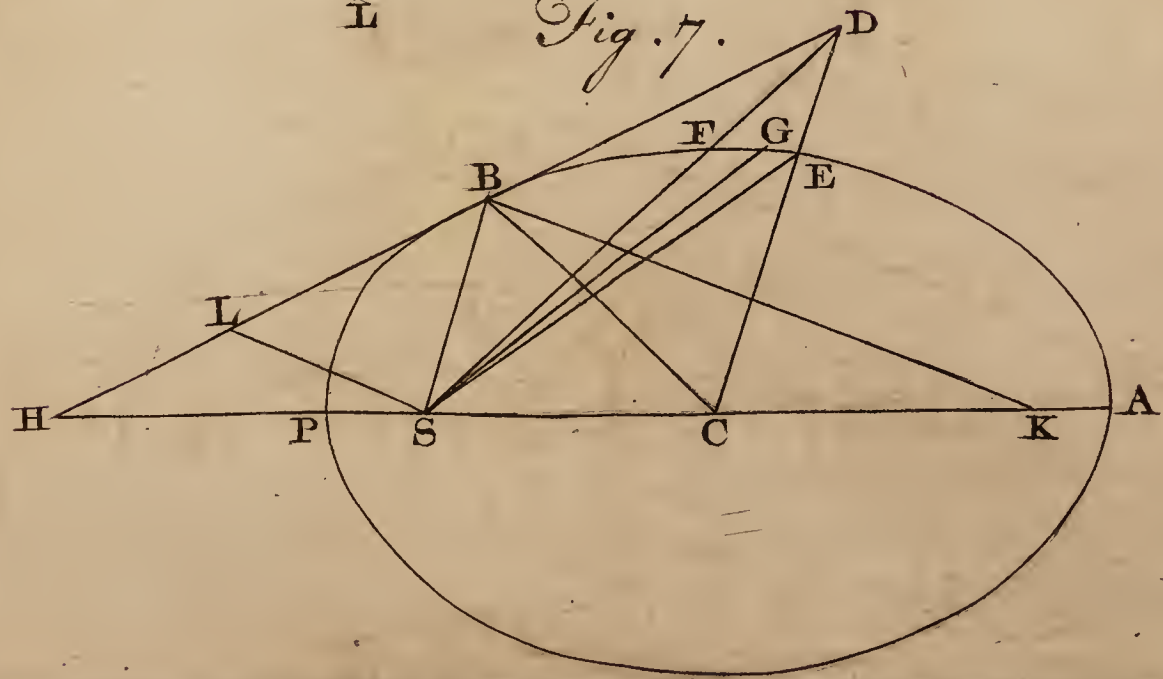


Fig. 7.





ART. VII.

Of the Cold produced by Evaporating Fluids, and of some other Means of producing Cold; by Dr. WILLIAM CULLEN Professor of Medicine in the University of Glasgow.*

A YOUNG Gentleman one of my pupils, whom I had employed to examine the heat or cold that might be produced by the solution of certain substances in spirit of wine, observed to me : That, when a thermometer had been immersed in spirit of wine, tho' the spirit was exactly of the temperature of the surrounding air, or somewhat colder; yet, upon taking the thermometer out of the spirit, and suspending it in the air, the mercury in the thermometer, which was of *Fahrenheit's* construction, always sunk two or three degrees. This recalled to my mind some experiments and observations of *M. de Mairan* to the same purpose; which I had read some time before. See *Dissertation sur la glace*, edit. 1749. pag. 248, & seq.

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* May 1. 1755.

When I first read the experiments of *M. de Miaran* in the place referred to, I suspected, that water, and perhaps other fluids, in evaporating, produced, or, as the phrase is, generated some degree of cold. The above experiment of my Pupil confirmed my suspicion, and engaged me to verify it by a variety of new trials.

I began by repeating the experiment with spirit of wine; and found, when I had taken the utmost care to have the spirit exactly of the temperature of the air, that constantly however, upon taking the thermometer out of the spirit, the mercury sunk several degrees, and indeed continued to sink so long as the ball of the thermometer continued wet with the spirit of wine. I found also, when the ball began to dry, and the mercury to rise again in the stem of the thermometer, that, if the ball was again dipped into the spirit and immediately taken out, the mercury in the thermometer might be again observed to sink, and that thus, by repeated dippings, the cold produced might be rendered very remarkable. The cold produced was also observed to be still greater, when, between each dipping, the thermometer was
 moved

moved very nimbly to and fro in the air ; or if, while the ball was wet with spirit of wine, it was blown upon by a pair of bellows ; or indeed if the air about the ball was otherwise any how put in motion. If any of these means for putting the air in motion are employed, the repeated moistenings of the ball of the thermometer may be performed by dipping it into the spirit of wine. But, when a certain degree of cold has been produced by a first dipping, that is apt to be diminished by dipping again into the warmer spirit ; and therefore the thermometer ought either to be dipped into the spirit and taken out again very quickly, or, what is still better, the ball of the thermometer ought to be moistened by a feather that has been dipped into the spirit of wine. By taking these methods, I have by spirit of wine made the mercury in the thermometer sink from 44 degrees to below the freezing point ; and, by employing some other fluids to be mentioned by and by, I have produced a sinking of the thermometer much more considerable.

IN making experiments of this kind, it is to be observed, that the cold produced is of very short duration. On this account it
is

is not proper to employ a thermometer inclosed in a glass tube; and it is necessary to employ one having a small ball, which may render it more sensible. But, as a small ball occasions the divisions of the scale to be the smaller, a thermometer filled with quicksilver is not so proper for these experiments, as one filled with spirit of wine; having at the same time both a small ball and a slender stem. What best of all shews the cold produced, and is indeed, with respect to several fluids, quite necessary, is an *Air* thermometer. This too will be rendered more convenient by having the upper part of the tube bent as in the figure annexed, so that the ball may be moistened without the liquor's running down upon the stem and scale.



I have entered into this detail for the sake of those who may desire to repeat my experiments. Having now said enough on the manner of making them, I go on to observe, that in this way I have examined a great variety of fluids. Such as,

The

The *quick-lime spirit* of *sal. ammoniac*,
 The *æther* of *Frobenius*,
 The *nitrous æther*,
 The *volatile tincture* of *sulphur*,
Spirit of *wine*,
Spirit of *sal. ammoniac* made with the *fixed*
alkali,
Brandy,
Wine,
Vinegar,
Water,
Oil of *turpentine*,
Oil of *mint*,
Oil of *pimento*.

By each of these employed to moisten the ball of the thermometer, some degree of cold is produced. I dare not however at present determine exactly what is the sinking of the thermometer produced by each. For this purpose, it would be necessary to repeat the trials often and with precisely the same circumstances at each time: which I find to be very difficult. In the mean time I have endeavoured to give a notion of the comparative power of these fluids in producing cold, by the order in which I have set them down; having mentioned that fluid first which seemed

seemed to me to occasion the greatest sinking of the thermometer, and the rest follow in order as they seemed to occasion less and less.

FROM the above enumeration I imagine it will appear, that the power of evaporating fluids in producing cold, is nearly according to the degree of volatility in each. If to this we join the consideration, that the cold is made greater by whatever hastens the evaporation, and particularly, that the sinking of the thermometer is greater as the air in which the experiment is made is warmer, if dry at the same time; I think we may now conclude, that *the cold produced is the effect of evaporation.*

I did not think it necessary to diversify my experiments further by examining a great many fluids, which are manifestly of a like nature with these above-mentioned. I presume pretty confidently, that the several spirituous, watery, and oily fluids, akin to these already tried, will be found all of them to have similar effects. And, considering how many fluids these classes comprehend, and that, in these already tried, the cold produced seems to depend more on the volatility of the
aggregate

aggregate than on the nature of the mixt; I was ready to conclude, that all fluids whatever, would, in evaporating, produce cold. But I have found a seeming exception. When the ball of the thermometer is moistened with any of the fossil acids, a considerable degree of heat is produced. It is however to be doubted, if this affords an exception. We know that these acids attract water from the air ; and also that these acids, mixed with water, always produce heat : it may therefore be supposed, that the heat produced, by moistening the ball of the thermometer with these acids, is to be imputed rather to their mixing with the water of the air, than to their evaporation singly. This perhaps cannot be positively determined, till the evaporation of these acids, in a very perfect vacuum, is examined ; which I have not yet had an opportunity of doing. In the mean time, I have made an experiment which I think is to the purpose. To one part of strong acid of vitriol, I added two parts of spring-water. When this mixture, which produces a great degree of heat, was returned to the temperature of the air, I used it for moistening the ball of the thermometer,

ter, and found it produced a sensible degree of cold, and seemingly a greater than water alone would have done. I need not here observe, that the mixture I used was still a very acid liquor, only so much saturated with water, that it would not now attract any from the air. Whether it would not have had the same effect, tho' less diluted, I have not had time to examine. The experiment, as it stands, tends to prove, that the heat produced by acids, applied to the ball of the thermometer, is owing to the mixture of these with the water of the air; and therefore, it is still very probable, that all fluids, which do not immediately affect the mixture of the air, will, in evaporating, produce cold.

WHEN I had proceeded thus far, I began to consider, whether the cold produced in the above experiments might not be the effect of the mixture of the several fluids with the air; and that therefore, to a list of cooling mixtures and solutions which I was then making up, I should now add the several solutions made by the air. By one who supposes the evaporation of fluids to depend upon the action of the air as a menstruum, this would be readily admitted; but, as I knew that

fluids

fluids evaporate *in vacuo* as well as in the air, I resolved to suspend my opinion, till I should repeat my experiments in an exhausted receiver.

IN prosecuting these, a number of new and to me curious *phænomena* have presented themselves ; so many, that I find the experiments must be often repeated, and much diversified, before I can give the Society a proper account of them. In the mean time, I shall give you the following facts already sufficiently verified.

A thermometer hung in the receiver of an air-pump, sinks always two or three degrees upon the air's being exhausted. After a little time, the thermometer *in vacuo* returns to the temperature of the air in the chamber, and upon letting air again into the receiver, the thermometer always rises two or three degrees above the temperature of the external air.

WHEN a vessel containing spirit of wine, with a thermometer immersed in it, is set under the receiver of an air-pump ; upon exhausting the air, the mercury in the thermometer sinks several degrees. It becomes more especially remarkable when the air in

any plenty issues out of the spirit of wine. As the spirit continues long to give out air, so it is long before the thermometer immersed in it returns to the temperature of the external air. But when after some time it appears stationary, if it is then drawn up out of the spirits and suspended in the vacuum, the mercury sinks very quickly eight or nine degrees; a good deal farther than it would have done in the same circumstances in the air. In the same manner as in the air, the thermometer *in vacuo* may be made to sink lower by repeated dippings into the spirit of wine: but here these repeated dippings have not so remarkable an effect as in the air; because the dipping cannot be so quickly performed, and the thermometer is therefore more affected by the warmth of the spirit. It is sometimes also affected by a drop of the spirit which the ball takes up along with it, and which, as I should have observed above, ought always to be taken away in the experiments made in the air. This experiment with spirit of wine was often enough repeated, to shew clearly, that the evaporation of the spirit

in vacuo produces a greater degree of cold than the evaporation of the same in the air.

SATISFIED of this, I have tried also some other fluids, as the quick-lime spirit of sal. ammoniac, and the two kinds of æther. Vessels containing these with a thermometer immersed in the liquor, but separately and at different times, were set under the receiver. With regard to the different fluids, the *phænomena* were much the same. Upon exhausting the receiver, the fluid gave out a great quantity of elastic air; and while this happened, the mercury in the immersed thermometer sunk very fast and to a great length. In our trials, it generally sunk below the scale applied so that we could not measure exactly how far. In one experiment before exhausting the receiver, the thermometer had stood at 50 degrees, and, after exhausting, we could judge very certainly that it sunk to below 20. In another experiment made with the nitrous æther, when the heat of the air was about 53 degrees, we set the vessel containing the æther in another a little larger containing water. Upon exhausting the receiver, and the vessel's remaining for a few minutes *in vacuo*, we found

found the most part of the water frozen, and the vessel containing the æther surrounded with a thick and firm crust of ice.

SUCH a means of producing cold, and to so great a degree, has not, so far as I know, been observed before, and it seems to deserve being further examined by experiments. Till that is done, I do not chuse to give any account of some other remarkable *phænomena* that have occurred in the above experiments, nor to enter into the several speculations that the subject seems to suggest.

SINCE writing the above, I have had occasion to observe, that Mr. *Richman* of the Academy of *Petersburg* has taken notice of the effect of evaporating fluids in producing cold; but does not impute it to the evaporation alone. His very exact account of the *phænomena*, and his theory with regard to them, may be seen in *Nov. Comment. Acad. Petropolitane ad ann. 1747 & 1748.* page 284.

ART. VIII.

*Experiments upon Magnesia alba, Quicklime,
and some other Alcaline Substances; by*
JOSEPH BLACK, M. D. *

PART I.

HOFFMAN, in one of his observations, gives the history of a powder called *magnesia alba*, which had been long used and esteemed as a mild and tasteless purgative; but the method of preparing it was not generally known before he made it public †.

IT was originally obtained from a liquor called the *mother of nitre*, which is produced in the following manner:

SALT-PETRE is separated from the brine which first affords it, or from the water with which it is washed out of nitrous earths, by the process commonly used in crystallizing salts. In this process the brine is gradually diminished, and at length reduced to a small quantity of an unctuous bitter saline liquor,
affording

* June 5. 1755.

† Hoff. op. T. iv. p. 479.

affording no more salt-petre by evaporation ; but, if urged with a brisk fire, drying up into a confused mass which attracts water strongly, and becomes fluid again when exposed to the open air.

To this liquor the workmen have given the name of the *mother of nitre* ; and *Hoffman*, finding it composed of the *magnesia* united to an acid, obtained a separation of these, either by exposing the compound to a strong fire in which the acid was dissipated and the *magnesia* remained behind, or by the addition of an alkali which attracted the acid to itself : and this last method he recommends as the best. He likewise makes an inquiry into the nature and virtues of the powder thus prepared ; and observes, that it is an absorbent earth which joins readily with all acids, and must necessarily destroy any acidity it meets in the stomach ; but that its purgative power is uncertain, for sometimes it has not the least effect of that kind. As it is a mere insipid earth, he rationally concludes it to be purgative only when converted into a sort of neutral salt by an acid in the stomach, and that its effect is therefore proportional to the quantity of this acid.

ALTHO'

ALTHO' *magnesia* appears from this history of it to be a very innocent medicine, yet having observed, that some hypochondriacs who used it frequently, were subject to flatulencies and spasms, he seems to have suspected it of some noxious quality. The circumstances however which gave rise to his suspicion, may very possibly have proceeded from the imprudence of his patients, who, trusting too much to *magnesia*, (which is properly a palliative in that disease,) and neglecting the assistance of other remedies, allowed their disorder to increase upon them. It may indeed be alledged, that *magnesia*, as a purgative, is not the most eligible medicine for such constitutions, as they agree best with those that strengthen, stimulate and warm; which the saline purges commonly used are not observed to do. But there seems at least to be no objection to its use when children are troubled with an acid in their stomach; for gentle purging in this case is very proper, and it is often more conveniently procured by means of *magnesia* than of any other medicine, on account of its being intirely insipid.

THE above-mentioned Author observing, some time after, that a bitter saline liquor, similar

similar to that obtained from the brine of salt-petre, was likewise produced by the evaporation of those waters which contain common salt, had the curiosity to try if this would also yield a *magnesia*. The experiment succeeded : and he thus found out another process for obtaining this powder, and at the same time assured himself by experiments, that the product from both was exactly the same*.

My curiosity led me some time ago to inquire more particularly into the nature of *magnesia*, and especially to compare its properties with those of the other absorbent earths, of which there plainly appeared to me to be very different kinds, altho' commonly confounded together under one name. I was indeed led to this examination of the absorbent earths, partly by the hope of discovering a new sort of lime and lime-water, which might possibly be a more powerful solvent of the stone than that commonly used ; but was disappointed in my expectations.

I have had no opportunity of seeing *Hoffman's* first *magnesia* or the liquor from which
it

* Hoff. Op. T. iv. p. 500.

it is prepared, and have therefore been obliged to make my experiments upon the second.

IN order to prepare it, I at first employed the bitter saline liquor called *bittern*, which remains in the pans after the evaporation of sea water. But as that liquor is not always easily procured, I afterwards made use of a salt called *epsom-salt*, which is separated from the *bittern* by crystallization, and is evidently composed of *magnesia* and the vitriolic acid.

THERE is likewise a spurious kind of Glauber salt, which yields plenty of *magnesia*, and seems to be no other than the *epsom salt* of sea water reduced to crystals of a larger size. And common salt also affords a small quantity of this powder; because being separated from the *bittern* by one hasty crystallization only, it necessarily contains a portion of that liquor.

THOSE who would prepare a *magnesia* from *epsom-salt*, may use the following process.

DISSOLVE equal quantities of *epsom-salt*, and of pearl ashes separately in a sufficient quantity of water; purify each solution from its dregs, and mix them accurately together

by violent agitation: then make them just to boil over a brisk fire.

ADD now to the mixture three or four times its quantity of hot water; after a little agitation, allow the *magnesia* to settle to the bottom, and decant off as much of the water as possible. Pour on the same quantity of cold water; and, after settling, decant it off in the same manner. Repeat this washing with the cold water ten or twelve times: or even oftner, if the *magnesia* be required perfectly pure for chemical experiments.

WHEN it is sufficiently washed, the water may be strained and squeezed from it in a linen cloth; for very little of the *magnesia* passes thro'.

THE alkali in the mixture uniting with the acid, separates it from the *magnesia*; which not being of itself soluble in water, must consequently appear immediately under a solid form. But the powder which thus appears is not intirely *magnesia*; part of it is the neutral salt, formed from the union of the acid and alkali. This neutral salt is found, upon examination, to agree in all respects with vitriolated tartar, and requires a large quantity of hot water to dissolve it. As much of it is therefore dissolved

dissolved as the water can take up; the rest is dispersed thro' the mixture in the form of a powder. Hence the necessity of washing the *magnesia* with so much trouble; for the first affusion of hot water is intended to dissolve the whole of the salt, and the subsequent additions of cold water to wash away this solution.

THE caution given of boiling the mixture is not unnecessary; if it be neglected, the whole of the *magnesia* is not accurately separated at once; and by allowing it to rest for some time, that powder concretes into minute grains, which, when viewed with the microscope, appear to be assemblages of needles diverging from a point. This happens more especially when the solutions of the epsom-salt and of the alkali are diluted with too much water before they are mixed together. Thus, if a dram of epsom-salt and of salt of tartar be dissolved each in four ounces of water, and be mixed, and then allowed to rest three or four days, the whole of the *magnesia* will be formed into these grains. Or if we filtrate the mixture soon after it is made, and heat the clear liquor which passes thro'; it will become turbid, and depofite a *magnesia*.

I had the curiosity to satisfy myself of the purgative power of *magnesia*, and of *Hoffman's* opinion concerning it, by the following easy experiment. I made a neutral salt of *magnesia* and distilled vinegar; choosing this acid as being, like that in weak stomachs, the product of fermentation. Six drams of this I dissolved in water, and gave to a middle-aged man, desiring him to take it by degrees. After having taken about a third, he desisted, and purged four times in an easy and gentle manner. A woman of a strong constitution got the remainder as a brisk purgative, and it operated ten times without causing any uneasiness. The taste of this salt is not disagreeable, and it appears to be rather of the cooling than of the acrid kind.

HAVING thus given a short sketch of the history and medical virtues of *magnesia*, I now proceed to an account of its chemical properties. By my first experiments, I intended to learn what sort of neutral salts might be obtained by joining it to each of the vulgar acids; and the result was as follows.

MAGNESIA is quickly dissolved with violent effervescence, or explosion of air, by the
acids

acids of vitriol, nitre, and of common salt, and by distilled vinegar; the neutral saline liquors thence produced having each their peculiar properties.

THAT which is made with the vitriolic acid, may be condensed into crystals similar in all respects to epsom-salt.

THAT which is made with the nitrous is of a yellow colour, and yields saline crystals, which retain their form in a very dry air, but melt in a moist one.

THAT which is produced by means of spirit of salt, yields no crystals; and if evaporated to dryness, soon melts again when exposed to the air.

THAT which is obtained from the union of distilled vinegar with *magnesia*, affords no crystals by evaporation, but is condensed into a saline mass, which, while warm, is extremely tough and viscid, very much resembling a strong glue both in colour and consistence, and becomes brittle when cold.

By these experiments *magnesia* appears to be a substance very different from those of the calcarious class; under which I would be understood to comprehend all those that are converted into a perfect quick-lime in a strong

strong fire, such as *lime-stone*, *marble*, *chalk*, those *spars* and *marles* which effervesce with *aqua fortis*, all *animal shells* and the bodies called *lithophyta*. All of these, by being joined with acids, yield a set of compounds which are very different from those we have just now described. Thus, if a small quantity of any calcarious matter be reduced to a fine powder and thrown into spirit of vitriol, it is attacked by this acid with a brisk effervescence; but little or no dissolution ensues. It absorbs the acid, and remains united with it in the form of a white powder, at the bottom of the vessel, while the liquor has hardly any taste, and shews only a very light cloud upon the addition of an alkali*.

THE same white powder is also formed when spirit of vitriol is added to a calcarious earth dissolved in any other acid; the vitriolic expelling the other acid, and joining

* Mr. *Margraaf* has lately demonstrated, by a set of curious and accurate experiments, that this powder is of the nature, and possesses the properties, of the gypseous or selenitic substances. That such substances can be resolved into vitriolic acid and calcarious earth, and can be again composed by joining these two ingredients together. *Mem de l'Acad. de Berlin*. an. 1750, p. 144.

joining itself to the earth by a stronger attraction ; and upon this account the *magnesia* of sea-water seems to be different from either of those described by *Hoffman*. He says, expressly, that the solutions of each of his powders, or, what is equivalent, that the liquors from which they are obtained, formed a coagulum, and deposited a white powder, when he added the vitriolic acid*; which experiment I have often tried with the marine bittern, but without success. The coagulum thus formed in the mother of nitre may be owing to a quantity of quick-lime contained in it ; for quick-lime is used in extracting the salt-petre from its matrix. But it is more difficult to account for the difference between *Hoffman's* bittern and ours, unless we will be satisfied to refer it to this, that he got his from the waters of salt springs, which may possibly be different from those of the sea.

MAGNESIA is not less remarkably distinguished from the calcarious earths, by joining it to the nitrous and vegetable acids, than to the vitriolic. Those earths, when combined with spirit of nitre, cannot be reduced to a crystalline

* Hoff. Op. T. iv. p. 480 & 500.

crystalline form, and if they are dissolved in distilled vinegar, the mixture spontaneously dries up into a friable salt.

HAVING thus found *magnesia* to differ from the common alkaline earths, the object of my next inquiry was its peculiar degree of attraction for acids, or what was the place due to it in Mr. *Geoffroy's* table of elective attractions.

THREE drams of *magnesia* in fine powder, an ounce of salt ammoniac, and six ounces of water were mixed together, and digested six days in a retort joined to a receiver.

DURING the whole time, the neck of the retort was pointed a little upwards, and the most watery part of the vapour, which was condensed there, fell back into its body. In the beginning of the experiment, a volatile salt was therefore collected in a dry form in the receiver, and afterwards dissolved into spirit.

WHEN all was cool, I found in the retort a saline liquor, some undissolved *magnesia*, and some salt ammoniac crystallized. The saline liquor was separated from the other two, and then mixed with the alkaline spirit. A
coagulum

coagulum was immediately formed, and a *magnesia* precipitated from the mixture.

THE *magnesia* which had remained in the retort, when well washed and dried, weighed two scruples and fifteen grains.

WE learn by the latter part of this experiment, that the attraction of the volatile alkali for acids is stronger than that of *magnesia*, since it separated this powder from the acid to which it was joined. But it also appears, that a gentle heat is capable of overcoming this superiority of attraction, and of gradually elevating the alkali, while it leaves the less volatile acid with the *magnesia*.

DISSOLVE a dram of any calcarious substance in the acid of nitre or of common salt, taking care that the solution be rendered perfectly neutral, or that no superfluous acid be added. Mix with this solution a dram of *magnesia* in fine powder, and digest it in the heat of boiling water about twenty four hours; then dilute the mixture with double its quantity of water, and filtrate. The greatest part of the earth now left in the filtre is calcarious, and the liquor which passed thro', if mixed with a dissolved alkali,

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yields a white powder, the largest portion of which is a true *magnesia*.

FROM this experiment it appears, that an acid quits a calcarious earth to join itself to *magnesia*; but the exchange being performed slowly, some of the *magnesia* is still undissolved, and part of the calcarious earth remains yet joined to the acid.

WHEN a small quantity of *magnesia* is thrown into a solution of the corrosive sublimate of mercury, it soon separates part of the mercury in the form of a dark red powder, and is itself dissolved.

IMAGINING that I perceived some resemblance between the properties of *magnesia* and those of alkalis, I was led to try what change this substance would suffer from the addition of quick-lime, which alters in such a peculiar manner the alkaline salts.

TWENTY seven grains of *magnesia* in fine powder were mixed with eighteen ounces of lime-water in a flask, which was corked close and shaken frequently for four days. During this time, I frequently dipp'd into it little bits of paper, which were coloured with the juice of violets; and these became green as soon as they touched the water, until the
fourth

fourth day, when their colour did not seem to be altered. The water being now poured off, was intirely insipid, and agreed in every chemical trial with pure water. The powder, after being perfectly well dried, weighed thirty seven grains. It did not dissolve intirely in spirit of vitriol; but, after a brisk effervescence, part of it subsided in the same manner as the calcarious earths, when mixed with this acid.

WHEN I first tried this experiment, I was at the trouble of digesting the mixture in the heat of boiling water, and did not then know that it would succeed in the heat of the air. But Dr. *Alston*, who has obliged the world with many curious and useful discoveries on the subject of quick-lime, having had occasion to repeat it, I learned from him that heat is not necessary; and he has moreover added an useful purpose to which this property of *magnesia* may be applied; I mean the sweetening of water at sea, with which lime may have been mixed to prevent its putrefaction.

THAT part of the dried powder which does not dissolve in spirit of vitriol, consists of the lime separated from the water.

QUICK-LIME

QUICK-LIME itself is also rendered mild by *magnesia*, if these two are well rubbed together and infused with a small quantity of water.

By the following experiments, I proposed to know whether this substance could be reduced to a quick-lime.

AN ounce of *magnesia* was exposed in a crucible for about an hour to such a heat as is sufficient to melt copper. When taken out, it weighed three drams and one scruple, or had lost $\frac{7}{12}$ of its former weight.

I repeated, with the *magnesia* prepared in this manner, most of those experiments I had already made upon it before calcination, and the result was as follows.

IT dissolves in all the acids, and with these composes salts exactly similar to those described in the first set of experiments; but what is particularly to be remarked, it is dissolved without any the least degree of effervescence.

IT slowly precipitates the corrosive sublimate of mercury in the form of a black powder.

IT separates the volatile alkali in salt ammoniac from the acid, when it is mixed
with

with a warm solution of that salt. But it does not separate an acid from a calcarious earth, nor does it induce the least change upon lime-water.

LASTLY, when a dram of it is digested with an ounce of water in a bottle for some hours, it does not make any the least change in the water. The *magnesia*, when dried, is found to have gained ten grains; but it neither effervesces with acids, nor does it sensibly affect lime-water.

OBSERVING *magnesia* to lose such a remarkable proportion of its weight in the fire, my next attempts were directed to the investigation of this volatile part, and, among other experiments, the following seemed to throw some light upon it.

THREE ounces of *magnesia* were distilled in a glass retort and receiver, the fire being gradually increased until the *magnesia* was obscurely red hot. When all was cool, I found only five drams of a whitish water in the receiver, which had a faint smell of the spirit of hartshorn, gave a green colour to the juice of violets, and rendered the solutions of corrosive sublimate and of silver very slightly turbid.

turbid. But it did not sensibly effervesce with acids.

THE *magnesia*, when taken out of the retort, weighed an ounce, three drams, and thirty grains, or had lost more than the half of its weight. It still effervesced pretty briskly with acids, tho' not so strongly as before this operation.

THE fire should have been raised here to the degree requisite for the perfect calcination of *magnesia*. But even from this imperfect experiment, it is evident, that of the volatile parts contained in that powder, a small proportion only is water; the rest cannot, it seems, be retained in vessels, under a visible form. Chemists have often observed, in their distillations, that part of a body has vanished from their senses, notwithstanding the utmost care to retain it; and they have always found, upon further inquiry, that subtile part to be air, which having been imprisoned in the body, under a solid form, was set free and rendered fluid and elastic by the fire. We may therefore safely conclude, that the volatile matter, lost in the calcination of *magnesia*, is mostly air; and hence the calcined *magnesia*

nesia does not emit air, or make an effervescence, when mixed with acids.

THE water, from its properties, seems to contain a small portion of volatile alkali, which was probably formed from the earth, air, and water, or from some of these combined together; and perhaps also from a small quantity of inflammable matter which adhered accidentally to the *magnesia*. Whenever Chemists meet with this salt, they are inclined to ascribe its origin to some animal, or putrid vegetable, substance; and this they have always done, when they obtained it from the calcarious earths, all of which afford a small quantity of it. There is, however, no doubt that it can sometimes be produced independently of any such mixture, since many fresh vegetables and tartar afford a considerable quantity of it. And how can it, in the present instance, be supposed, that any animal or vegetable matter adhered to the *magnesia*, while it was dissolved by an acid, separated from this by an alkali, and washed with so much water?

Two drams of *magnesia* were calcined in a crucible, in the manner described above, and thus reduced to two scruples and twelve grains.

grains. This calcined *magnesia* was dissolved in a sufficient quantity of spirit of vitriol, and then again separated from the acid by the addition of an alkali, of which a large quantity is necessary for this purpose. The *magnesia* being very well washed and dried, weighed one dram and fifty grains. It effervesced violently, or emitted a large quantity of air, when thrown into acids, formed a red powder when mixed with a solution of sublimate, separated the calcarious earths from an acid, and sweetened lime-water : and had thus recovered all those properties which it had but just now lost by calcination : nor had it only recovered its original properties, but acquired besides an addition of weight nearly equal to what had been lost in the fire ; and, as it is found to effervesce with acids, part of the addition must certainly be air.

THIS air seems to have been furnished by the alkali from which it was separated by the acid ; for Dr. *Hales* has clearly proved, that alkaline salts contain a large quantity of fixed air, which they emit in great abundance when joined to a pure acid. In the present case, the alkali is really joined to an acid, but without any visible emission of air ; and yet
the

the air is not retained in it: for the neutral salt, into which it is converted, is the same in quantity, and in every other respect, as if the acid employed had not been previously saturated with *magnesia*, but offered to the alkali in its pure state, and had driven the air out of it in their conflict. It seems therefore evident, that the air was forced from the alkali by the acid, and lodged itself in the *magnesia*.

THESE considerations led me to try a few experiments, whereby I might know what quantity of air is expelled from an alkali, or from *magnesia*, by acids.

Two drams of a pure fixed alkaline salt, and an ounce of water, were put into a Florentine flask, which, together with its contents, weighed two ounces and two drams. Some oil of vitriol diluted with water was dropt in, until the salt was exactly saturated; which it was found to be, when two drams, two scruples, and three grains of this acid had been added. The vial with its contents now weighed two ounces, four drams, and fifteen grains. One scruple, therefore, and eight grains were lost during the ebullition, of which a trifling portion may be water, or

something of the same kind. The rest is air.

THE celebrated *Homberg* has attempted to estimate the quantity of solid salt contained in a determined portion of the several acids. He saturated equal quantities of an alkali with each of them ; and, observing the weight which the alkali had gained, after being perfectly dried, took this for the quantity of solid salt contained in that share of the acid which performed the saturation. But we learn from the above experiment, that his estimate was not accurate, because the alkali loses weight as well as gains it.

Two drams of *magnesia*, treated exactly as the alkali in the last experiment, were just dissolved by four drams, one scruple, and seven grains of the same acid liquor, and lost one scruple and sixteen grains by the ebullition.

Two drams of *magnesia* were reduced, by the action of a violent fire, to two scruples and twelve grains, with which the same process was repeated, as in the two last experiments ; four drams, one scruple, and two grains of the same acid were required to complete

pleat the solution, and no weight was lost in the experiment.

As in the separation of the volatile from the fixed parts of bodies, by means of heat, a small quantity of the latter is generally raised with the former; so the air and water, originally contained in the *magnesia*, and afterwards dissipated by the fire, seem to have carried off a small part of the fixed earth of this substance. This is probably the reason, why calcined *magnesia* is saturated with a quantity of acid, somewhat less than what is required to dissolve it before calcination: and the same may be assigned as one cause which hinders us from restoring the whole of its original weight, by solution and precipitation.

I took care to dilute the vitriolic acid, in order to avoid the heat and ebullition which it would otherwise have excited in the water; and I chose a Florentine flask, on account of its lightness, capacity, and shape, which is peculiarly adapted to the experiment; for the vapours raised by the ebullition circulated for a short time, thro' the wide cavity of the vial, but were soon collected upon its sides, like dew, and none of them seemed to reach
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the neck, which continued perfectly dry to the end of the experiment.

WE now perceive the reason, why crude and calcined *magnesia*, which differ in many respects from one another, agree however in composing the same kind of salt, when dissolved in any particular acid; for the crude *magnesia* seems to differ from the calcined chiefly by containing a considerable quantity of air, which air is unavoidably dissipated and lost during the dissolution.

FROM our experiments, it seems probable, that the increase of weight which some metals acquire, by being first dissolved in acids, and then separated from them again by alkalis, proceeds from air furnished by the alkalis. And that in the *aurum fulminans*, which is prepared by the same means, this air adheres to the gold in such a peculiar manner, that, in a moderate degree of heat, the whole of it recovers its elasticity in the same instant of time; and thus, by the violent shock which it gives to the air around, produces the loud crack or fulmination of this powder. Those who will imagine the explosion of such a minute portion of fixed air, as can reside in the *aurum fulminans*, to be insufficient for the
excessive

excessive loudness of the noise, will consider, that it is not a large quantity of motion communicated to the air, but rather a smart stroke which produces sound, and that the explosion of but a few particles of fixed air may be capable of causing a loud noise, provided they all recover their spring suddenly, and in the same instant.

THE above experiments lead us also to conclude, that volatile alkalis, and the common absorbent earths, which lose their air by being joined to acids, but shew evident signs of their having recovered it, when separated from them by alkalis, received it from these alkalis which lost it in the instant of their joining with the acid.

THE following are a few experiments upon three of the absorbent earths, made in order to compare them with one another, and with *magnesia*.

Suspecting that *magnesia* might possibly be no other than a common calcarious earth, which had changed its nature, by having been previously combined with an acid, I saturated a small quantity of chalk with the muriatic acid, separated the acid from it again by
means

means of a fixed alkali, and carefully washed away the whole of the salt.

THE chalk when dried was not found to have suffered any alteration ; for it effervesced with the vitriolic acid, but did not dissolve in it ; and when exposed to a violent fire, was converted into a quick-lime, in all respects similar to that obtained from common chalk.

IN another experiment of the same kind, I used the vitriolic acid with the same event.

ANY calcarious matter reduced to a fine powder, and thrown into a warm solution of alum, immediately raises a brisk effervescence. But the powder is not dissolved ; it is rather increased in bulk : and if the addition be repeated until it is no longer accompanied with effervescence, the liquor loses all taste of the alum, and yields only a very light cloud upon the admixture of an alkali.

FROM this experiment we learn, that acids attract the calcarious earths more strongly than they do the earth of alum ; and as the acid in this salt is exactly the same with the vitriolic, it composes with the calcarious earth a neutral substance, which is very difficultly soluble in water, and therefore falls
down

down to the bottom of the vessel along with the earth of alum which is deprived of its acid. The light cloud formed by the alkali proceeds from the minute portion of the calcarious compound which saturates the water.

THE earth of animal bones, when reduced to a fine powder and thrown into a diluted vitriolic acid, gradually absorbs the acid in the same manner as the calcarious earths, but without any remarkable effervescence. When it is added to the nitrous or to the muriatic acid, it is slowly dissolved. The compound liquor thence produced is extremely acrid, and still changes the colour of the juice of violets to a red, even after it is fully saturated with the absorbent. Distilled vinegar has little or no effect upon this earth; for after a long digestion it still retains its sour taste, and gives only a light cloud upon the addition of an alkali.

By dropping a dissolved fixed alkali into a warm solution of alum, I obtained the earth of this salt, which, after being well washed and dried, was found to have the following properties.

IT

IT is dissolved in every acid but very slowly, unless assisted by heat. The several solutions, when thoroughly saturated, are all astringent with a slight degree of an acid taste, and they also agree with a solution of alum in this, that they give a red colour to the infusion of turnsol.

NEITHER this earth, nor that of animal bones, can be converted into quick-lime by the strongest fire, nor do they suffer any change worth notice. Both of them seem to attract acids but weakly, and to alter their properties less when united to them than the other absorbents.

PART II.

IN reflecting afterwards upon these experiments, an explication of the nature of lime offered itself, which seemed to account, in an easy manner, for most of the properties of that substance.

IT is sufficiently clear, that the calcarious earths in their native state, and that the alkalis and magnesia in their ordinary condition, contain a large quantity of fixed air, and
this

this air certainly adheres to them with considerable force, since a strong fire is necessary to separate it from magnesia, and the strongest is not sufficient to expell it entirely from fixed alkalis, or take away their power of effervescing with acid salts.

THESE considerations led me to conclude, that the relations between fixed air and alkaline substances was somewhat similar to the relation between these and acids ; that as the calcarious earths and alkalis attract acids strongly and can be saturated with them, so they also attract fixed air, and are in their ordinary state saturated with it : and when we mix an acid with an alkali or with an absorbent earth, that the air is then set at liberty, and breaks out with violence ; because the alkaline body attracts it more weakly than it does the acid, and because the acid and air cannot both be joined to the same body at the same time.

I also imagined, that, when the calcarious earths are exposed to the action of a violent fire, and are thereby converted into quicklime, they suffer no other change in their composition than the loss of a small quantity of water and of their fixed air. The re-

markable acrimony which we perceive in them after this process, was not supposed to proceed from any additional matter received in the fire, but seemed to be an essential property of the pure earth, depending on an attraction for those several substances which it then became capable of corroding or dissolving, which attraction had been insensible as long as the air adhered to the earth, but discovered itself upon the separation.

THIS supposition was founded upon an observation of the most frequent consequences of combining bodies in chemistry. Commonly when we join two bodies together, their acrimony or attraction for other substances becomes immediately either less perceivable or entirely insensible; altho' it was sufficiently strong and remarkable before their union, and may be rendered evident again by disjoining them. A neutral salt, which is composed of an acid and alkali, does not possess the acrimony of either of its constituent parts. It can easily be separated from water, has little or no effect upon metals, is incapable of being joined to inflammable bodies, and of corroding and dissolving animals and vegetables; so that the attraction both of the acid and alkali for these several substances

substances seems to be suspended till they are again separated from one another.

CRUDE lime was therefore considered as a peculiar acrid earth rendered mild by its union with fixed air: and quick-lime as the same earth, in which, by having separated the air, we discover that acrimony or attraction for water, for animal, vegetable, and for inflammable substances.

THAT the calcarious earths really lose a large quantity of air when they are burnt to quick-lime, seems sufficiently proved by an experiment of Mr. *Margraaf**, an exceedingly accurate and judicious Chemist. He subjected eight ounces of *osteocolla* to distillation in an earthen retort, finishing his process with the most violent fire of a reverberatory, and caught in the receiver only two drams of water, which by its smell and properties shewed itself to be slightly alkaline. He does not tell us the weight of the *osteocolla* remaining in the retort, and only says, that it was converted into quick-lime; but as no calcarious earth can be converted into quick-lime, or bear the heat which he applied without losing above a third of its weight, we may safely

* Mem. de l'Acad. de Berlin. an. 1748, p. 57.

safely conclude, that the loss in his experiment was proportional, and proceeded chiefly from the dissipation of fixed air.

ACCORDING to our theory, the relation of the calcarious earth to air and water appeared to agree with the relation of the same earth to the vitriolic and vegetable acids. As chalk for instance has a stronger attraction for the vitriolic than for the vegetable acid, and is dissolved with more difficulty when combined with the first, than when joined to the second: so it also attracts air more strongly than water, and is dissolved with more difficulty when saturated with air than when compounded with water only.

A calcarious earth deprived of its air, or in the state of quick-lime, greedily absorbs a considerable quantity of water, becomes soluble in that fluid, and is then said to be slaked; but as soon as it meets with fixed air, it is supposed to quit the water and join itself to the air, for which it has a superior attraction, and is therefore restored to its first state of mildness and insolubility in water.

WHEN slaked lime is mixed with water, the fixed air in the water is attracted by
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the lime, and saturates a small portion of it, which then becomes again incapable of dissolution, but part of the remaining flaked lime is dissolved and composes lime-water.

IF this fluid be exposed to the open air, the particles of quick-lime which are nearest the surface gradually attract the particles of fixed air which float in the atmosphere. But at the same time that a particle of lime is thus saturated with air, it is also restored to its native state of mildness and insolubility; and as the whole of this change must happen at the surface, the whole of the lime is successively collected there under its original form of an insipid calcarious earth, called the cream or crusts of lime-water.

WHEN quick-lime itself is exposed to the open air, it absorbs the particles of water and of fixed air which come within its sphere of attraction, as it meets with the first of these in greatest plenty, the greatest part of it assumes the form of flaked lime; the rest is restored to its original state; and if it be exposed for a sufficient length of time, the whole of it is gradually saturated with air, to which the water as gradually yields its place.

WE

WE have already shewn by experiment, that magnesia alba is a compound of a peculiar earth and fixed air. When this substance is mixed with lime-water, the lime shews a stronger attraction for fixed air than that of the earth of magnesia ; the air leaves this powder to join itself to the lime. And as neither the lime when saturated with air, nor the magnesia when deprived of it, are soluble in water, the lime-water becomes perfectly pure and insipid, the lime which it contained being mixed with the magnesia. But if the magnesia be deprived of air by calcination before it is mixed with the lime-water, this fluid suffers no alteration. To prove

IF quick-lime be mixed with a dissolved alkali, it likewise shews an attraction for fixed air superior to that of the alkali. It robs this salt of its air, and thereby becomes mild itself, while the alkali is consequently rendered more corrosive, or discovers its natural degree of acrimony or strong attraction for water, and for bodies of the inflammable, and of the animal and vegetable kind ; which attraction was less perceivable as long as it was saturated with air. And the volatile alkali when deprived of its air, besides this attraction

attraction for various bodies, discovers likewise its natural degree of volatility, which was formerly somewhat repressed by the air adhering to it, in the same manner as it is repressed by the addition of an acid.

THIS account of lime and alkalis recommended itself by its simplicity, and by affording an easy solution of many *phænomena*, but appeared upon a nearer view to be attended with consequences that were so very new and extraordinary, as to render suspicious the principles from which they were drawn.

I resolved however to examine, in a particular manner, such of these consequences as were the most unavoidable, and found, the greatest number of them might be reduced to the following propositions:

I. IF we only separate a quantity of air from lime and alkalis, when we render them caustic they will be found to lose part of their weight in the operation, but will saturate the same quantity of acid as before, and the saturation will be performed without effervescence.

II. IF

II. IF quick-lime be no other than a calcareous earth deprived of its air, and whose attraction for fixed air is stronger than that of alkalis, it follows, that, by adding to it a sufficient quantity of alkali saturated with air, the lime will recover the whole of its air, and be entirely restored to its original weight and condition: and it also follows, that the earth separated from lime-water by an alkali, is the lime which was dissolved in the water now restored to its original mild and insoluble state.

III. IF it be supposed that flaked lime does not contain any parts which are more firey, active or subtile than others, and by which chiefly it communicates its virtues to water; but that it is an uniform compound of lime and water: it follows, that, as part of it can be dissolved in water, the whole of it is also capable of being dissolved.

IV. IF the acrimony of the caustic alkali does not depend on any part of the lime adhering to it, a caustic or soap-ley will consequently be found to contain no lime, unless the quantity of lime employed in making it were greater than what is just sufficient to extract the whole air of the alkali; for then

as much of the superfluous quick-lime might possibly be dissolved by the ley as would be dissolved by pure water, or the ley would contain as much lime as lime-water does.

V. WE have shewn in the former experiments, that absorbent earths lose their air when they are joined to an acid ; but recover it, if separated again from that acid, by means of an ordinary alkali : the air passing from the alkali to the earth, at the same time that the acid passes from the earth to the alkali.

IF the caustic alkali therefore be destitute of air, it will separate magnesia from an acid under the form of a magnesia free of air, or which will not effervesce with acids ; and the same caustic alkali will also separate a calcarious earth from acids under the form of a calcarious earth destitute of air, but saturated with water, or under the form of flaked lime.

THESE were all necessary conclusions from the above suppositions. Many of them appeared too improbable to deserve any further attention : some however, I found upon reflection, were already seconded by experience. Thus *Hoffman* has observed, that quicklime does not effervesce with spirit of

vitriol* ; and it is well known that the caustic spirit of urine, or of salt ammoniac, does not emit air, when mixed with acids. This consideration excited my curiosity, and determined me to inquire into the truth of them all by way of experiment. I therefore engaged myself in a set of trials ; the history of which is here subjoined. Some new facts are likewise occasionally mentioned ; and here it will be proper to inform the reader, that I have never mentioned any without satisfying myself of their truth by experiment, tho' I have sometimes taken the liberty to neglect describing the experiments when they seemed sufficiently obvious.

DESIRING to know how much of an acid a calcarious earth will absorb, and what quantity of air is expelled during the dissolution, I saturated two drams of chalk with diluted spirit of salt, and used the Florentine flask, as related in a similar experiment upon magnesia. Seven drams and one grain of the acid finished the dissolution, and the chalk lost two scruples and eight grains of air.

THIS experiment was necessary before the following, by which I proposed to inquire
into

* Hoff. Op. T. iv. p. 480.

into the truth of the first proposition so far as it relates to quick-lime.

Two drams of chalk were converted into a perfect quick-lime, and lost two scruples and twelve grains in the fire. This quick-lime was flaked or reduced to a milky liquor with an ounce of water, and then dissolved in the same manner, and with the same acid, as the two drams of chalk in the preceding experiment. Six drams, two scruples and fourteen grains of the acid finished the saturation without any sensible effervescence or loss of weight.

It therefore appears from these experiments, that no air is separated from quick-lime by an acid, and that chalk saturates nearly the same quantity of acid after it is converted into quick-lime as before.

With respect to the second proposition, I tried the following experiments.

A piece of perfect quick-lime made from two drams of chalk, and which weighed one dram and eight grains, was reduced to a very fine powder, and thrown into a filtrated mixture of an ounce of a fixed alkaline salt and two ounces of water. After a slight digestion, the powder being well washed and dried,

dried, weighed one dram and fifty eight grains. It was similar in every trial to a fine powder of ordinary chalk, and was therefore saturated with air which must have been furnished by the alkali.

A dram of pure salt of tartar was dissolved in fourteen pounds of lime-water, and the powder thereby precipitated, being carefully collected and dried, weighed one and fifty grains. When exposed to a violent fire, it was converted into a true quick-lime, and had every other quality of a calcarious earth.

THIS experiment was repeated with the volatile alkali, and also with the fossil or alkali of sea-salt, and exactly with the same event.

THE third proposition had less appearance of probability than the foregoing; but, as an accurate experiment was the only test of its truth, I reduced eight grains of perfect quick-lime made of chalk, to an exceedingly subtile powder, by flaking it in two drams of distilled water boiling hot, and immediately threw the mixture into eighteen ounces of distilled water in a flask. After shaking it, a light sediment, which floated thro' the liquor, was allowed to subside; and this, when collected

collected with the greatest care, and dried, weighed, as nearly as I could guess, one third of a grain. The water tasted strongly of the lime, had all the qualities of lime-water, and yielded twelve grains of precipitate, upon the addition of salt of tartar. In repeating this experiment, the quantity of sediment was sometimes less than the above, and sometimes amounted to half a grain. It consisted partly of an earth which effervesced violently with *aqua fortis*, and partly of an ochry powder, which would not dissolve in that acid. The ochry powder, as it usually appears in chalk to the eye, in the form of veins running thro' its substance, must be considered only as an accidental or foreign admixture; and, with respect to the minute portion of alkaline earth which composed the remainder of the sediment, it cannot be supposed to have been originally different from the rest, and incapable, from its nature, of being converted into quick-lime, or of being dissolved in water; it seems rather to have consisted of a small part of the chalk in its mild state, or saturated with air, which had either remained, for want of a sufficient fire to drive it out entirely, or had been furnished by the distilled water.

I indeed expected to see a much larger quantity of sediment produced from the lime, on account of the air which water constantly contains, and with a view to know whether water retains its air when fully saturated with lime, a lime-water was made as strong as possible ; four ounces of which were placed under the receiver of an air-pump, together with four ounces of common water in a vial of the same size ; and, upon exhausting the receiver, without heating the vials, the air arose from each in nearly the same quantity : from whence it is evident, that the air, which quick-lime attracts, is of a different kind from that which is mixed with water. And that it is also different from common elastic air, is sufficiently proved by daily experience ; for lime-water, which soon attracts air, and forms a crust when exposed in open and shallow vessels, may be preserved, for any time, in bottles which are but slightly corked, or closed in such a manner as would allow free access to elastic air, were a vacuum formed in the bottle. Quick-lime therefore does not attract air when in its most ordinary form, but is capable of being joined to one particular species only, which is dispersed thro' the atmosphere,
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either in the shape of an exceedingly subtile powder, or more probably in that of an elastic fluid. To this I have given the name of fixed air, and perhaps very improperly; but I thought it better to use a word already familiar in philosophy, than to invent a new name, before we be more fully acquainted with the nature and properties of this substance, which will probably be the subject of my further inquiry.

It is, perhaps, needless to mention here, that the calcarious substances used in making the above experiments should be of the purest kind, and burnt with the utmost violence of heat, if we would be sure of converting them into perfect quick-lime. I therefore made use of chalk burnt in a small covered crucible with the fiercest fire of a Black-smith's forge, for half an hour, and found it necessary to employ, for this purpose, a crucible of the *Austrian* kind, which resemble black lead; for if any calcarious substance be heated to such a degree in an ordinary or *Hessian* crucible, the whole of it is melted down, together with part of the vessel, into glass.

I now prepared to inquire into the properties of the caustic alkali; in order to which,

I made a caustic or soap ley in the following manner.

TWENTY six ounces of very strong quicklime made of chalk, were flaked or reduced to a sort of fluid paste, with eleven pounds of boiling water, and then mixed in a glass vessel with eighteen ounces of a pure fixed alkaline salt, which had been first dissolved in two pounds and a half of water. This mixture was shaken frequently for two hours, when the action of the lime upon the alkali was supposed to be over, and nothing remained but to separate them again from one another. I therefore added 12 pounds of water, stirred up the lime, and, after allowing it to settle again, poured off as much of the clear ley as possible.

THE lime and alkali were mixed together under the form of a very thick milky liquor or fluid paste; because they are thus kept in perpetual contact and equal mixture until they have acted sufficiently upon one another: whereas in the common way of using a larger quantity of water, the lime lies for the most part at bottom, and, tho' stirred up ever so often, cannot exert its influence so fully upon
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the alkali, which is uniformly diffused thro' every part of the liquor.

THE above ley was found upon trial to be saturated by acids without the least effervescence or diminution of weight.

IT was now proper to examine whether the alkali suffered any loss in becoming caustic, which I proposed to attempt by ascertaining the strength of the ley, or the quantity of salt which a given portion of it contained; from which by computation some imperfect knowledge might be obtained of the quantity of caustic produced from the eighteen ounces of mild salt.

I therefore evaporated some of my ley, but soon perceived that no certain judgment could be formed of its strength in this way, because it always absorbed a considerable quantity of air during the evaporation, and the dried salt made a pretty brisk effervescence with acids, so that the ley appeared stronger than it really was; and yet, upon proceeding in the estimate from this rude and unfair trial, it appeared that the salt had lost above a sixth in becoming caustic, and the quantity of acid saturated by two drams of

it was to the quantity of acid saturated by two drams of salt of tartar, nearly as six to five.

THESE experiments are therefore agreeable to that part of the second proposition which relates to the caustic alkali.

UPON farther examining what changes the alkali had undergone, I found that the ley gave only an exceeding faint milky hue to lime-water; because the caustic alkali wants that air by which salt of tartar precipitates the lime. When a few ounces of it were exposed in an open shallow vessel for four and twenty hours, it imbibed a small quantity of air, and made a slight effervescence with acids. After a fortnight's exposure in the same manner, it became entirely mild, effervesced as violently with acids, and had the same effect upon lime-water as a solution of an ordinary alkali. It likewise agrees with lime-water in this respect, that it may be kept in close vessels, or even in bottles which are but slightly covered, for a considerable time, without absorbing a sensible quantity of air.

IN order to know how much lime it contained, I evaporated ten ounces in a small
silver

silver dish over a lamp, and melted the salt, after having dissipated the water*.

THE caustic thus produced was dissolved again in a small quantity of water, and deposited a trifling portion of sediment, which I imagined at first to be lime; but finding that it could easily be dissolved in a little more water, concluded it to be a vitriolated tartar, which always accompanies the fixed alkali of vegetables.

I then saturated the solution of the caustic salt with spirit of vitriol, expecting thus to detect the lime; because that acid precipitates a calcarious earth from its ordinary solutions. During the saturation, a large quantity of white powder was formed; but this likewise turned out to be a vitriolated tartar, which had appeared in the form of a powder, because there was not enough of water in the mixture to dissolve it.

LASTLY

* This evaporation was performed in a silver dish, on account of the acrimony of the salt; which is so very great, that, having once evaporated a part of the same ley in a bowl of English earthen or stone ware, and melted the caustic with a gentle heat, it corroded and dissolved a part of the bowl, and left the inside of it pitted with small holes.

LASTLY, I exposed a few ounces of the ley in an open shallow vessel so long, that the alkali lost the whole of its causticity, and seemed entirely restored to the state of an ordinary fixed alkali; but it did not however deposite a single atom of lime. And to assure myself that my caustic ley was not of a singular kind, I repeated the same experiments with an ordinary soap-ley, and with one made by mixing one part of a pure fixed alkaline salt with three parts of common stone lime fresh flaked and sifted; nor could I discover any lime in either. The first of these contained a small quantity of brimstone, and was far from being perfectly caustic, for it made a pretty brisk effervescence with acids; but the last was so entirely deprived of its air, that it did not diminish in the least the transparency of lime-water.

THESE experiments seem therefore to support the fourth proposition, and to shew that the caustic alkali does not contain any lime.

As it seems probable, from the quickness and ease wherewith the alkali was rendered caustic, that more lime had been employed than what was just sufficient to extract the whole
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of its air, we are surpris'd to find that little or none of the superfluous quick-lime was dissolved by the water. But this *phænomenon* will become less surprizing, by comparing it with some similar instances in chemistry. Water may be made to deposite a salt, by the admixture of a substance which it attracts more strongly than it does that salt; such as spirit of wine; and quick-lime itself may be separated from water upon the same principle; for if that spirit is added to an equal quantity of lime-water, the mixture becomes turbid and deposite a sediment, which, when separated and dissolved again in distilled water, composes lime-water. We may therefore refer the above *phænomenon*, with respect to the ley, to the same cause with these, and say, that the water did not dissolve the lime, because it already contained a caustic alkali, for which it has a superior attraction.

I also rendered the volatile alkali caustic, in order to examine what change it suffered in the operation, and obtained an exceedingly volatile and acrid spirit, which neither effervesced with acids, nor altered in the least the transparency of lime-water; and, altho' ve-
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ry strong, was lighter than water, and floated upon it like spirit of wine.

I next inquired into the truth of the fifth proposition, in the following manner.

Two drams of epsom-salt were dissolved in a small quantity of water, and thrown into two ounces of the caustic-ley ; the mixture instantly became thick, like a decoction of starch or barley, by the magnesia, which was precipitated. I then added spirit of vitriol by degrees, until the mixture became perfectly clear, or the whole of the magnesia was again dissolved ; which happened without any effervescence or emission of air.

HALF an ounce of chalk was dissolved in spirit of salt, the quantity of which was so adjusted, that the mixture was not acid in the least degree ; and the solution was thrown into twelve ounces of the caustic ley ; which quantity I found, by experiment, to be sufficient for precipitating almost the whole of the chalk. I now filtrated this turbid liquor, and laid the powder remaining in the paper upon a chalk-stone, in order to draw as much of the water from it as possible, and thereby reduce it to the form of a more dense and heavy powder, that it might subside the more perfectly

perfectly in the following part of the experiment. I then mixed it with about twenty ounces of pure water in a flask, and, after allowing the powder to subside, poured off the water, which had all the qualities of lime-water. And I successively converted eight waters more into lime-water, seven of these in the same quantity, and with the same management, as the first. The eighth was likewise in the same quantity; but I allowed it to remain with the chalk, and shook it frequently, for two days. This, after being filtrated, formed a cream or crust upon its surface when exposed to the air; changed the colour of the juice of violets into green; separated an orange-coloured powder from a solution of corrosive sublimate; became turbid upon the addition of an alkali; was entirely sweetened by magnesia; and appeared so strong to the taste, that I could not have distinguished it from ordinary lime-water. And when I threw some salt ammoniac into the lime which remained, the vapour of the volatile alkali immediately arose from the mixture.

In this experiment therefore the air is first driven out of the chalk by an acid, and then,
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in order to separate this acid from it, we add an alkali which has been previously deprived of its air ; by which means, the chalk itself is also obtained free of air, and in an acrid form, or in the form of flaked lime.

WE have also several processes for obtaining the volatile alkali in a caustic form, which seem to be only so many methods of obtaining it in its pure state, and free of fixed air. The first of these is the separation of the alkali from an acid, merely by heat ; an instance of which we have from Mr. *Margraaf**. He prepared from urine an ammoniacal salt, the acid of which is the basis of the phosphorus, and is of such a peculiar nature, that it endures a red heat without being dissipated. Sixteen ounces of the neutral salt were subjected by him to distillation. The acid remained in the retort, and he found in the receiver eight ounces of an alkaline spirit, which, he tells us, was extremely volatile, very much resembling the spirit of salt ammoniac distilled with quick-lime ; and no crystals were formed in it, when exposed to the cold air.

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* Mem. de l'Acad. de Berlin, an. 1746, p. 87.

A caustic volatile alkali may also be obtained, by mixing salt ammoniac with half its weight of a caustic fixed alkali, or of magnesia which has been previously deprived of its air by fire ; and then submitting these mixtures to distillation : Or merely by adding any ordinary volatile alkali to a proper quantity of a caustic ley ; for in this case the air passes from the volatile to the fixed alkali, by a superior attraction for the last, and, by a gentle heat, the compound yields a spirit similar to that prepared from salt ammoniac and quick-lime.

It is therefore probable, that, had we also a method of separating the fixed alkali from an acid, without, at the same time, saturating it with air, we should then obtain it in a caustic form ; but I am not acquainted with an instance of this separation in chemistry. There are two indeed which, at first sight, appear to be of this kind ; these are the separation of the fixed alkali from the nitrous acid by means of inflamed charcoal, in the process for making *nitrum fixatum*, and of the same alkali, from vegetable acids merely by heat ; but, upon examining the product of each process, we find the alkali either

fully or nearly saturated with air. In the first, either the charcoal or the acid, or both together, are almost wholly converted into air; a part of which is probably joined to the alkali. In the second, the acid is not properly separated, but rather destroyed by the fire: a considerable portion of it is converted into an inflammable substance; and we learn from *Dr. Hales*, that the bodies of this class contain a large quantity of fixed air.

WHEN we consider that the attraction of alkalis for fixed air is weaker than that of the calcarious earths, and reflect upon the effects of heat in chemistry, we are led to imagine, that alkalis might be entirely deprived of their air, or rendered perfectly caustic, by a fire somewhat weaker than that which is sufficient to produce the same change upon lime; but this opinion does not seem agreeable to experience.

THE alkalis do, however, acquire some degree of causticity in a strong fire, as appears from their being more easily united with spirit of wine after having been kept in fusion for some time. For that fluid, which cannot be tinctured by a mild salt of tartar, will

will soon take a very deep colour from a few drops of a strong caustic ley. The circumstances which hinder us from rendering these salts perfectly caustic by heat, are their propensity to dissipation in the utmost violence of the fire, their extreme acrimony, and the imperfection of our common vessels. For before the heat becomes very intense, the alkalis either evaporate, or dissolve a part of the crucibles in which they are contained, and often escape thro' their pores; which happens, especially as soon as they have already acquired some degree of additional acrimony, by the loss of part of their air.

THE fusion also, which they so readily undergo, is well known by Chemists, as a strong obstacle to the separation of the volatile from the fixed parts of a compound by fire; accordingly, in several processes, we are directed to add to the fusible compound some porous substance which is incapable of fusion, and will retain the whole in a spongy form, thereby to facilitate the dissipation of the volatile parts.

IN order to know whether an alkali would lose a part of its air, and acquire a degree of causticity, when exposed, with this precaution, to the action of a strong fire, I mixed an
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ounce and a half of salt of tartar with three ounces of black-lead, a substance of any the most unchangeable by chemical operations. This mixture I exposed, for several hours, in a covered crucible, to a fire somewhat stronger than what is necessary to keep salt of tartar in fusion. When allowed to cool, I found it still in the form of a loose powder; and taking out one half, I diluted it with water, and by filtration obtained a ley, which, when poured into a solution of white marble in *aqua fortis*, precipitated the marble under the form of a weak quick-lime: for the turbid mixture gave a green colour to the juice of violets, and threw up a crust like that of lime-water; and the precipitated powder collected and mixed with salt ammoniac immediately yielded the scent of the volatile alkali.

LEST it should here be suspected, that the alkaline qualities of this mixture, and of the precipitated marble, were not owing to a lime into which the marble was converted, but to the alkali itself which was added, it is proper to observe, that I mixed so small a proportion of the ley with the solution of marble as made me sure, from certain experiments, that the whole of the alkali was spent in performing

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ing the precipitation, and was consequently converted into a neutral salt by attracting the acid. The properties therefore of the mixture can only be referred to a lime, as is indeed sufficiently evident from the crust which is peculiar to lime-water.

I was therefore assured by this experiment, that an alkali does really lose a part of its air, and acquire a degree of causticity, by the proper application of heat ; but finding by several trials, that the degree of causticity which it had thus acquired was but weak, and that the quick-lime produced in this experiment was exhausted and rendered mild by a small quantity of water, I exposed the crucible together with that half of the alkali which remained in it to a stronger fire, in order to expel a larger quantity of air, and render it more remarkably caustic ; but the whole of it was dissipated by the force of the heat, and the black lead, which still retained the form of a loose and subtile powder, yielded little or nothing to water.

WE learn then from the above experiment the reason why the alkali newly obtained from the ashes of vegetables is generally of the more acrid kinds of that salt. It never
appears

appears until the subject be converted into ashes, and is supposed to be formed by the fire, and to be the result of a particular combination of some of the principles of the vegetable; one of which principles is air, which is contained in large quantity in all vegetable matters whatever. But as soon as the smallest part of a vegetable is converted into ashes, and an alkali is thus formed, this salt necessarily suffers a calcination, during which it is kept in a spongy form by the ashes, and shews a very considerable degree of acrimony if immediately applied to the body of an animal; but if the ashes are for any time exposed to the air, or if we separate the alkali from them by the addition of a large quantity of water and subsequent evaporation, the salt imbibes fixed air from the atmosphere, and becomes nearly saturated with it: tho' even in this condition it is generally more acrid than salt of tartar, when this is prepared with a gentle heat.

BORAX has sometimes been referred to the class of alkalis, on account of some resemblance it bears to those salts: but it has been demonstrated by accurate experiments, that we should rather consider it as a neutral salt; that

that it is composed of an alkali and of a particular saline substance called the sedative salt, which adheres to the alkali in the same manner as an acid, but can be separated by the addition of any acid whatever, the added acid joining itself to the alkali in the place of the sedative salt. As this conjunction of an acid with the alkali of borax happens without the least effervescence, our principles lay us under a necessity of allowing that alkali to be perfectly free of air, which must proceed from its being incapable of union with fixed air and with the sedative salt at the same time: whence it follows, that, were we to mix the sedative salt with an alkali saturated with air, the air would immediately be expelled, or the two salts in joining would produce an effervescence. This I found to be really the case upon making the trial, by mixing a small quantity of the sedative salt with an equal quantity of each of the three alkalis, rubbing the mixtures well in a mortar, and adding a little water. It is however proper in this place to observe, that, if the experiments be made in a different manner, they are attended with a singular circumstance. If a small quantity of the sedative salt be thrown into a
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large proportion of a dissolved fixed alkali, the sedative salt gradually disappears, and is united to the alkali without any effervescence ; but if the addition be repeated several times, it will at last be accompanied with a brisk effervescence, which will become more and more remarkable, until the alkali be entirely saturated with the sedative salt.

THIS *phænomenon* may be explained by considering the fixed alkalis as not perfectly saturated with air : and the supposition will appear very reasonable, when we recollect, that those salts are never produced without a considerable degree of heat, which may easily be imagined to dissipate a small portion of so volatile a body as air. Now, if a small quantity of the sedative salt be thrown into an alkaline liquor, as it is very slowly dissolved by water, its particles are very gradually mixed with the atoms of the alkali. They are most strongly attracted by such of these atoms as are destitute of air, and therefore join with them without producing an effervescence ; or, if they expel a small quantity of air from some of the salt, this air is at the same time absorbed by such of the
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contiguous particles as are destitute of it, and no effervescence appears until that part of the alkali, which was in a caustic form or destitute of air, be nearly saturated with the sedative salt. But if, on the other hand, a large proportion of the sedative salt be perfectly and suddenly mixed with the alkali, the whole, or a large part, of the air is as suddenly expelled.

In the same manner may we also explain a similar *phænomenon*, which often presents itself in saturating an alkali with the different acids: the effervescence is less considerable in the first additions of acid, and becomes more violent as the mixture approaches the point of saturation. This appears most evidently in making the *sal diureticus* or regenerated tartar: The particles of the vegetable acid here employed being always diffused thro' a large quantity of water, are more gradually applied to those of the alkali, and during the first additions are chiefly united to those that are freest of air*.

VOL. II.

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* Boerh. Operat. Chem. process. LXXVI.

THAT the fixed alkali, in its ordinary state, is seldom entirely saturated with air, seems to be confirmed by the following experiment.

I exposed a small quantity of a pure vegetable fixed alkali to the air, in a broad and shallow vessel, for the space of two months; after which I found a number of solid crystals, which resembled a neutral salt so much as to retain their form pretty well in the air, and to produce a considerable degree of cold when dissolved in water. Their taste was much milder than that of ordinary salt of tartar; and yet they seemed to be composed only of the alkali, and of a larger quantity of air than is usually contained in that salt, and which had been attracted from the atmosphere: for they still joined very readily with any acid, but with a more violent effervescence than ordinary; and they could not be mixed with the smallest portion of vinegar, or of the sedative salt, without emitting a sensible quantity of air.

As it now appeared that several alkaline substances have an attraction for fixed air, I tried a few experiments to learn the relative strength of their several attractions.

TWENTY four grains of magnesia in fine powder were mixed with five ounces of the caustic ley in a small vial, which was immediately corked and shaken frequently for four hours. The ley was then poured off, and the magnesia washed with repeated affusions of water, and dried. It had lost about the half of its weight, and when reduced to a fine powder was readily dissolved by acids with an effervescence which was hardly perceivable : the alkali had therefore extracted its air. I also threw some fresh magnesia into the ley which had been poured off, and thereby rendered it perfectly mild and similar to a solution of salt of tartar ; so that it effervesced briskly with acids.

WITH an ounce of the mild spirit of salt ammoniac, I mixed a dram of magnesia in very fine powder which had been previously deprived of its air by fire ; and observing that the magnesia had a tendency to concrete into a solid mass, I shook the vial very frequently. After some days the powder was increased to more than double its former bulk ; and when the vial was opened, the alkaline spirit emitted a most intolerably pungent smell. It likewise floated upon water, but was not perfectly

perfectly caustic ; for it still yielded some air when mixed with acids, and also rendered lime-water turbid : neither of which would probably have happened if I had used a greater quantity of magnesia, or had allowed the mixture to remain a longer time in the vial. I now washed out the whole of the mixture into a bowl, and dried the magnesia until it lost all smell of the alkali. It weighed a dram and fifty eight grains, effervesced violently with acids, and therefore contained a large quantity of air, which had been drawn from the alkali by a stronger attraction.

HAVING formerly shewn, that magnesia saturated with air separates an acid from a calcarious earth, which it is not able to do after being deprived of its air by fire ; I now suspected that the air was the cause of this separation, because I found that it was joined to the calcarious earth at the same time that the acid was joined to the earth of magnesia ; and imagined that a pure calcarious earth might possibly have a stronger attraction for acids than a pure earth of magnesia.

I therefore dissolved two drams of magnesia in the marine acid, and thus obtained a
compound

compound of an acid and of the pure earth of this substance; for the air which was at first attached to it, was expelled during the dissolution. I then added thirty grains of strong quick-lime in exceeding fine powder, shook the mixture well, and filtrated it. The powder remaining in the paper, after being well washed, was found to be a magnesia, which, as I expected, was destitute of air; for it was dissolved by the vitriolic acid without effervescence. And the filtrated liquor contained the lime united to the acid; for upon dropping spirit of vitriol into it, a white powder was immediately formed.

WE must therefore acknowledge a stronger attraction between the calcarious earths and acids than between these and magnesia: but how does it then happen, that, if magnesia saturated with air be mixed with a compound of acid and calcarious earth, these two last, which attract one another the most strongly, do not remain united; but the acid is joined to the magnesia, and the calcarious earth to the air which it attracts much more weakly than it does the acid? Is it because the sum of the forces which tend to join the magnesia to the acid and the calcarious earth to the
air

air, is greater than the sum of the forces which tend to join the calcarious earth to the acid, and the magnesia to the air: and because there is a repulsion between the acid and air, and between the two earths; or they are somehow kept afunder in such a manner as hinders any three of them from being united together?

THE first part of this supposition is favoured by our experiments, which seem to shew a greater difference between the forces wherewith the calcarious earth and magnesia attract fixed air, than between those which dispose them to unite with the acid. The repulsions however hinted in the second are perhaps more doubtful, tho' they are suggested in many other instances of decomposition; but the bounds of my present purpose will not allow me to enter upon this subject, which is one of the most extensive in chemistry.

WE meet also with a difficulty with respect to the volatile alkali similar to the above. Thus a calcarious earth that is pure or free of air has a much stronger attraction for acids than a pure volatile alkali, as is evident when we mix quick-lime with salt-ammoniac;

for the alkali is then immediately detached from the acid: and agreeably to this I found, upon trial, that a pure or caustic volatile alkali does not separate a calcarious earth from an acid. Yet, if we mix a mild volatile alkali, which is a compound of alkali and air, with a compound of acid and calcarious earth, these two last, which attract one another most strongly, do not remain united; but the acid is joined to the alkali and the earth to the air, as happens in the precipitation of a calcarious earth from an acid, by means of the common or mild volatile alkali.

I remember likewise a parallel instance with regard to quick-silver. This metal has an attraction for the vitriolic acid, and when joined to it appears under the form of turbith mineral: but this attraction is weaker than that of the fixed alkali for the same acid; for if we mix a dissolved salt of tartar with turbith mineral, the turbith is converted into a brown powder, and the alkali into vitriolated tartar; which change happens the sooner, if the pure or caustic alkali is used. Yet, if to a compound of quick-silver and the nitrous acid, we add a compound of the fixed
alkali

alkali and the vitriolic acid, or a vitriolated tartar, and digest the mixture with a strong heat, the vitriolic acid does not remain with the alkali, but is joined to the quick-silver which it attracts more weakly, composing with it a turbith mineral; while the alkali is joined to the nitrous acid which it likewise attracts more weakly than it does the vitriolic, and is converted into salt-petre.

FROM some of the above experiments, it appears, that a few alterations may be made in the column of acids in Mr. *Geoffroy's* table of elective attractions, and that a new column may be added to that table, according to the following scheme, where the alkaline substances are all considered as in their pure state and free of fixed air.

Acids.	Fixed air.
Fixed alkali,	Calcareous earth.
Calcareous earth,	Fixed alkali.
Volatile alkali and magnesia,	Magnesia.
* * * * *	Volatile alkali.
	* * * *

AT

AT the foot of the first column several of the metals might follow, and after these the earth of alum; but as I don't know what number of the metals should precede that earth, I have left it to be determined by further experience.

THE volatile alkali and magnesia are placed in the same line of this column; because their force of attraction seems pretty equal. When we commit a mixture of magnesia and salt ammoniac to distillation, the alkali arises and leaves the acid with the magnesia; because this earth, by attracting the acid, represses its volatility, and it seems also to diminish the cohesion of the acid and alkali, and to render them separable by a gentle heat. If the magnesia be saturated with air, this likewise, on account of its volatile nature and attraction for the alkali, is driven up along with it, and makes it appear under a mild form, and in the same manner do the alkali and air arise from a mixture of salt ammoniac and of a crude calcarious earth.

ART. IX.

Of the Analysis and Uses of Peat, by ALEXANDER LIND, Esquire *.

The Analysis of Peat.

FIVE ounces of peat, hard, dry, and of a deep brown colour, found six or seven miles from *Edinburgh*, being distilled with a gradual heat, gave first an ounce and an half of clear water; after which the oil began to come over, first yellow, then darker coloured, till it became like tar, and along with it, a yellow or brown coloured acid oily liquor, strongly empyreumatic, to the quantity of six drams: this liquor, towards the last, became alkaline: but of this there was only a small quantity; for the distilled liquor that came over first, was found to be considerably acid, so that it took more than a dram of *ol. tart. per deliquium* to saturate it; if the distillation had been continued with a very violent heat, there would have come over more alkaline spirit with the pitchy oil: the quantity of oil

* 1744.

oil that came over was two drams and an half ; it coagulates in the cold, and melts with a small heat. There remained in the retort two ounces of coal ; which being carefully burnt in a crucible, left eleven drams and a few grains of fine yellow ashes.

THAT it may appear how far the *Scotch* and *Dutch* peats agree, I shall next give the *analysis* of some *Dutch* peat made by *Degner*. He took twenty four ounces of peat, which being first reduced to a powder ; and then put in a retort, yielded, by a gentle distillation, a good quantity of insipid phlegm, with an empyreumatical smell. This being taken away, there followed next a yellow spirit, and about the neck of the retort, remained sticking a certain white unctuous earth, resembling a volatile salt. The degree of heat being increased, there came forth a white smoke, with which a red oil rising at the same time, was collected in the receiver, in the form of a thick pitchy matter, swimming upon the spirit, and sticking strongly to the sides of the vessel. In the retort remained only a black coal.

HAVING weighed each of these separately, the coal was nine ounces six drams ; the
thick

thick pitchy oil, an ounce and an half; the volatile oily salt or spirit four ounces; the rest, the insipid phlegm received in an open receiver.

THE phlegm being first examined, was nothing but a simple clear watery liquor, without taste or smell.

THE redish yellow liquor that came next, was a liquid volatile unctuous salt, composed of a watery phlegm, a volatile salt, and a little oil, which commonly goes under the name of spirit. This mixt with spirit of sea salt occasioned only a few bubbles; it raised with oil of vitriol an effervescence, and turned muddy; it precipitated a solution of mercury in *aqua fortis*, into a black powder; turned syrop of violets green, had no effect upon chalk, spirit of salt ammoniac: spirit of harts-horn made no other change upon it, but turning the liquor, which was before muddy, clear. From all which, he says, it appears, this spirit is of an alkaline nature; and that except a little oil, contains chiefly a volatile alkaline salt.

THE oil which was found swimming upon the water or spirit had a strong empyreumatical smell, tho' not so fetid as the distilled oil
of

of animals. yet stronger than that of oils distilled from bituminous mineral substances, and was in smell the most like that of peat smoke. In the cold, it congealed into a body of the consistence of soap or honey, or rather into a pitchy substance of a redish brown colour, being of a caustic pungent taste; upon the fire it melted like wax, and when cold, looked like the foot of peat.

THIS oil easily catches fire, but is soon again extinguished. When kindled, it burns like weak spirit of wine, but not so intensely. When put upon the fire in a spoon, if you approach a lighted candle, it kindles into a flame; and, upon the removal of the candle, is immediately extinguished; so it must be frequently kindled before it wholly consumes.

THE cinder that remained in the retort, being put in the fire, in a short time turned red, and immediately, without smoke or flame, fell into grey ashes, nothing different from ordinary peat-ashes. These ashes contain less salt than those of wood. All peat-ashes do not yield the same quantity of salt, but differ according to the quality of the peat. From a pound of *Dutch* ashes, *Degner*, after boiling, filtrating, and inspissating in the ordinary

nary way, obtained only half an ounce of a redish impure salt, mixt with much earth : from other peats that leave after burning a red ash, and that were taken from dryer ground, he had an ounce of a redish impure salt.

THIS salt, he says, has rather a saline than alkaline taste ; and when exposed to the open air, runs slowly, after the manner of such saline lixivious salt. Being dissolved in water and mixed with oil of vitriol, it becomes turbid, with a small bubbling up. With fixed salt, or spirit of harts-horn, it turns muddy ; with spirit of sea salt, there is no change, the liquor remaining clear, only a few bubbles. It coagulates soap when boiled with it, in the same manner that sea salt does. This salt dissolved again in water, filtrated and inspissated until it begins to crystallize, gathers, when set in a cool place, into a salt of a cubical form, and when thrown into the fire, makes a noise like sea salt that is decrepitated. The liquor being further inspissated and set to crystallize a second time, yielded a nitrous salt ; the remaining liquor was a pure lixivious alkaline salt, that made a strong effervescence with acids. From all which it appears,

pears, that this salt is principally composed of a salt like that of common sea salt, with a small proportion of the alkaline kind.

What remained after the salt was extracted out of the ashes, was only a slimy earth and some sand.

To these analyses, I shall add two others. Dr. *Smidberg* a Physician at the *Hague*, having distilled some peat in a retort, obtained from it a spirit, an oil, a volatile salt, all of them much like that of hartshorn. *Ottman* an Apothecary at *Stutberg*, from a *Swabian* peat, had a volatile spirit, like that of spirit of tartar, a fetid oil, but no volatile salt.

The Uses of Peat.

THE principal use of peat is burning, not only for the service of families, but likewise for that of a great many trades; such as brewers, bakers, distillers, making of lime, &c.: and, as there is a good deal of difference in peats, and some kinds preferred to others, I shall here take notice of some of the principal differences.

THE

THE first is, with respect to the place out of which they are taken. Such as are got from brackish grounds, near the sea, also such as are impregnated with vitriol or sulphur, have a disagreeable smell, and are hurtful to the health. In *Zealand*, they have a kind of peat, which, when burning, makes every body in the room look like a dead person; and, when they sit long by the fire, grow faintish: it also turns the bottom of their vessels white. Peats taken from mosses, free of all minerals, have none of the above mentioned, or any other bad effect.

As to the matter itself, that differs in many respects; so that in the same moss, according to the different depth of it, there are three or four different kinds of peats found. In *North Britain*, in the province of *Groningen*, and in several other places, that which is uppermost, is light and spungy; further down, better; and at bottom, is a substance that is black, and makes a firm solid peat.

In *Holland*, that which lies uppermost is best, being of a dark or black colour, to which others succeed of different colours and substances not so good. That which is light and spungy, taken from a barren heathy ground,

ground, or from a dry sandy soil ; also such moss as is much mixed with pieces of rotten wood, roots, mud, gravel, or sand, or which consuming quickly, leaves behind a great many impurities mixed with its ashes, is bad.

PEATS differ considerably, according to the pains bestowed in making them. Such as are perfectly freed from all heterogeneous matter, well knead and wrought, are the best of all. Upon which account, peats, made in the province of *Holland*, where no labour is spared in the working them, are preferable to all others ; tho', in other places, the substance may be equally good. A *Dutch* peat six inches long and three or four thick, will weigh a pound ; a peat made at *Nimiguen*, of the same dimensions, will not weigh above half a pound, often less. It is a general observation, that all peats made of moss-mud, and well knead, are considerably heavier than such as are only cut out of the moss.

PEATS that are of a dark colour, and solid, that continue longest in the fire without consuming, that have a good cinder, and fall into white ashes, are most esteemed : on the

contrary, such as are light and porous, consume quickly in the fire, leave no cinder, but a great many impurities and ashes, are little valued.

THE ashes of peats differ also considerably in colour, quantity, and weight ; as to which nothing certain can be determined. Sometimes the white, sometimes the grey, and at other times the red, are heaviest. In *Friesland*, the peat that leaves the red ashes, *Degner* says, is heaviest ; about *Nimiguen* those that are red are found lighter than the *Dutch* peat which leaves a grey ash.

THE Brewer, Distiller, and other trades, prefer the peat that leaves a red ash, which, tho' it seldom has a firm cinder, yet burns violently. The Baker makes choice of the light turf, and in *North-Holland*, where the inhabitants are extremely cleanly, they use the peat that has red ashes, upon account of their being heavier than the white, and therefore not so apt to fly about and spoil their furniture.

BESIDES the uses now commonly made of peats, there are two others in which, I think, they may be employed with great advantage. The first is, the smelting iron ore, the only
fewel

fewel at present used in that operation is charcoal of oak, and other hard woods ; any attempt made to do it with pit-coal, so far as I can learn, has hitherto proved unsuccessful ; and indeed from the nature of that substance, there seems little hopes of ever bringing it to answer the end, the bituminous or inflammable part of pit-coal having nearly the same effect upon iron, which common sulphur has. It destroys, as experience shews, the malleability of iron and all other metals. Pit-coal has likewise another bad quality, which I have often found to my cost. With a strong heat it runs into a glassy substance, which in time, by its sticking so closely together, and to the sides of the furnace, quite choaks it up, and, by its tenacity, hinders the metallic parts from sinking downwards, as they would do by their natural gravity. What is chiefly wanted in smelting, is an open fire ; the furnaces are commonly sufficiently clogged with the stony and other heterogeneous bodies united with the ore, which run into glass without the addition of any such foreign matter as has a tendency to vitrification. The char'd wood, on the contrary, keeps always an open fire, the inflammable

flamable part of which is so far from hurting metals, that it preserves their malleability, by supplying with its own sulphur that which is destroyed or carried off from the metal, by the intense heat employed in the smelting ; and this is so far true, that iron reduced to a *calx* or friable substance by calcination, has its malleability restored by being fluxed with powdered charcoal only. And this we may here observe, by the bye, as an instance of the difference of substituting a vegetable in place of a mineral sulphur.

THE peat then being intirely a vegetable substance, there seems nothing more requisite to make it a proper fuel for smelting iron, but the being able to raise by its means a heat sufficient for that purpose. This, experience shews, cannot be done with the peats we now have. The most likely method of obtaining this end, I think, is, to bring them to be as solid and compact a substance as possible. The densest bodies, *cæteris paribus*, when thoroughly heated, are, the hottest: hence it is, that metals as they are the heaviest bodies, so they reach the greatest degree of heat. The same holds in fuel ; the hardest woods are made choice of, when

when a strong heat is wanted ; and even in common peats, I have shewn you how far preferable the hard and solid are to the light and spungy. By some experiments which I have made, I find it to be no difficult matter, to bring peat to a considerable degree of solidity, as you yourselves may see by the specimen I now show you. The simple operation of grinding, does the business ; and as a peat, when taken out of the moss, is a soft body, and easily grinded, a machine may be easily contrived to grind, at a moderate expence, several tuns in a day. The charge of digging peats, cutting them into squares or the form of bricks, when of a proper dryness, will be little different from that of making peats in the ordinary way. The solidity of peat prepared in the manner mentioned is surprising ; its specific gravity being somewhat greater than that of pit-coal. I compared a peat of this kind with a piece of coal brought from Baron *Clerk's* coal-mines near *Edinburgh*, and by the hydrostatical balance, reckoning water 1000, their specific gravities were nearly as follows, pit-coal 1287, solid peat 1303.

FROM

FROM what has been said, it appears, that, if iron could be made with peat, it would be of great service, particularly in some places of *North Britain*, where peat is to be had in plenty, along with iron, which now lies unwrought for want of wood : and even where wood may be found, if peat brought to the consistency I mention would do the business, it would come cheaper than char'd wood. Another advantage of this kind of peat, would be the smelting of lead with it alone, which cannot well be done at present, without the help of pit-coal, which in some places must be brought from a considerable distance, and at no small charge.

THE other use I would propose of peat, is the employing it as dung, for the fertilizing of ground, when prepared in the manner I shall afterwards mention. I am not ignorant, that the ashes of peats are used for that purpose with great advantage, not only by themselves, but likewise mixed with other dung ; and even the dust of peat, that remains at the bottom of peat-stacks ; but in that state it has not the effects of dung, nor are its effects equal to what they would be, were it rightly prepared. To set this matter
in

in a proper light, I must be allowed to say something in general of vegetation, and of the use of dungs in promoting it.

VEGETABLES which increase by seed, as is the case with by far the greatest part, if not all of them, are at first *plantulas* wrapt up in a very small bulk in the end of the seed ; which, when put in the ground, by the moisture they find there, extend themselves, and are first nourished by part of the seed itself, which does the same office to the young plant, by affording it a finer nourishment, as the *placenta* does to the *embryo*. When the plant becomes stronger and shoots forth its roots, it then draws its nourishment from the earth. Thus it goes on growing until it has attained its utmost perfection : after which it gradually decays, dies, and at last rots and putrefies. By putrefaction, the parts of which the vegetable was composed, *viz.* its salts, oils, phlegm and earth, are separated : part remains upon the ground where the plant falls ; but the far greatest part being volatile, flies up into the air, from whence it descends again upon the earth and incorporates with it. The same materials serve to nourish new plants, there being no part of them, as we all know,

know, lost. What we call a vegetable mould, is an earth in which there is store of such parts of vegetables lodged, the *matrix* in which they lie, being a fine but barren sand. As long as there is a sufficient stock of such particles in any earth, that ground is fruitful ; but when this is exhausted, which happens sooner or later, from the quantity of vegetables nourished by it, and carried off for the uses of life, it becomes barren. The only remedy, when no better can be had, is to allow it to rest, until it receives a new recruit from the air, in which are perpetually floating, and falling down upon the earth, particles of all kinds, proper for the nourishment of plants. But as this is a tedious way of recovering the fertility of ground, the better and more expeditious one, is by laying dung upon it, which being wholly made up of putrefied vegetables, or animals, equally proper for nourishing plants, the parts, of which both are composed, being the same, and the transition from the one to the other easy ; the ground by this new acquisition becomes again fertile. Every vegetable then whose parts are set loose, by that last fermentation of nature, putrefaction, affords

affords a proper *pabulum* for vegetables ; and the great distinction of plants, which commonly lies in a very small part, and that too the most volatile, being taken away by putrefaction, all vegetables, when reduced to that state, seem to be pretty much upon a *par* for that purpose. Now, to return to what I intended to say, and to which what I have mentioned was only a kind of preamble ; Peat moss, being wholly a vegetable matter, must, if reduced to a thorough state of putrefaction, answer the same purposes for fertilizing ground as other putrefied vegetables. While it lies in the moss, there is too great a quantity of water, to raise a sufficient degree of heat, to bring the vegetables of which peat-moss is composed, whether actually growing, decaying, or decayed, to a compleat degree of putrefaction. But if it were taken out of the moss, and laid in heaps like other vegetables to rot, with a degree of moisture suitable for that purpose ; and if, to begin and also quicken the putrefaction, green fresh succulent plants were employed in a sufficient quantity first to raise a heat ; this I make no doubt would, by communicating it to the mossy substance, in a suitable

time, and by right management, reduce the whole mafs to the ftate defired. This already is in fome measure practifed in *Holland*, where they mix the duft of peats with ordinary dung, in making of dung-hills.

I fhall conclude this paper with only mentioning two others ufes of peat, which I had almoft forgot, *viz.* that peat-duft ftrowed upon ground where peafe or other feeds are fown, in order to have an early crop, is an excellent prefervative of fuch vegetables from the froft; as it keeps the ground warm, by not allowing the cold to penetrate into it. And that there is nothing properer than peat to ftop water, and to confine it, in the making of fifh-ponds, &c. This I learned from his Grace the Duke of *Argyle*, who I obferved ufed it with great fuccefs for that purpofe.

ART.

ART. X.

*The Effects of Semen Hyoscyami albi, by
Dr. ARCHIBALD HAMILTON Physician in
Edinburgh *.*

————— Student of phyfic, of a thin habit of body, about twenty years of age, had been, for two years, in the use of taking a small quantity of white henbane-feed to make him sleep, and without any bad effect. But on *Friday March* the 8th 1754, betwixt four and five in the afternoon, he, in order to procure sleep, swallowed about twice as much of this feed as he could take up betwixt his fore-finger and thumb, *i. e.* nearly 25 grains. He felt himself half an hour after, very heavy and much inclined to sleep ; his eyes were oppressed, and spirits dejected, with a general lassitude and inactivity over his whole body. These symptoms still increasing, he went abroad and drank tea about six o'clock ; and with great difficulty could keep awake, having sometimes let fall the tea-spoon insensibly. He complained of a great uneasiness
and

* May 1. 1755.

and dryness of his throat, and that the tea in swallowing was like to choak him. He had also some gripes in his belly. After he drank tea, he was soon seized with convulsions and so great a degree of insensibility, that he did not know the people who were in company with him. He spoke many incoherent things, and at the beginning of his illness said he was afraid he had taken poison. The people who were with him being alarmed, ordered him to be carried home, and sent for me. I found him incapable of giving any account of his misfortune. His eyes were open and rolling, now and then he was seized with tremors, startings, and convulsions, grappling the bed-cloaths, his head, face, nose, and other parts of his body, in the manner patients frequently do in nervous fevers. His pulse excessively small and low with *subfultus tendinum*. The sense of feeling seemed also impaired; for when I pinched his skin, he made no complaint. He had no inclination to vomit, nor had any stool from the time he took the feed. I ordered him immediately a vomit; and in the mean time Dr. Boswell was sent for. He spit out the vomit as soon as it was poured in-

to his mouth ; so that it appeared he was either not deprived of taste, or could not let the vomit over. A second vomit was instantly given, which was also spilt or spit out. A solution of white vitriol was afterwards given, the most of which he swallowed without the desired effect. A strong clyster with antimonial wine was administered, which he kept about twenty minutes. He got a second injection an hour after. As he continued in the same miserable situation, a blister was applied to his head, and sinapisms to the soles of his feet. He passed the night in the same condition without sleeping, and was alternately seized with convulsions, startings, and caught with his hands at every thing about him. In the morning, he became more sensible, and began to speak a little distinctly, altho' his head was yet very confused and muddy. He told what quantity of the seed he had taken, and for what purpose. His pulse was now somewhat stronger. He got a purgative infusion, which operated four times that day. In the evening, he was still more distinct, altho' his eyes continued heavy and his head

head confused. His pulse was now quick, full, and strong; and he complained of a great pain and weight in his head. He was blooded about twelve o'clock that night to the quantity of twelve ounces. He sweated plentifully, had good sleep; and was altogether sensible and distinct next morning.

ART.

ART. XI.

*The Effects of the Thorn-Apple, by Dr. ABRAHAM SWAINE Physician at Brentford *.*

ROBERT BULMER, a man of a strong constitution, 69 years of age, and who had enjoyed a good state of health all his life, till about two years before, when he was first afflicted with the gravel ; in *October* 1746, being advised by a friend to take a decoction of the fruit of the common burdock, as a remedy for his disease, by mistake gathered the fruit of the *stramonium* or thorn-apple. After dividing three of these, each of which was as big as a small hen's egg, into two parts, he boiled them in a pint of milk, which, when a little cooled, he drank off about eight o'clock in the morning fasting. Presently afterwards, he became vertiginous or giddy ; and therefore rose from his chair to take the air, with an intention to pluck more fruit. In walking two or three hundred yards from his house, he staggered as
if

* May 1. 1755.

if drunk, feared he should fall on his head, and that he was about to lose his senses ; but had no sickness nor the least inclination to vomit. As soon as he got home he went to bed, and complaining of an excessive dryness of his tongue and throat, a little water mixed with wine was given him ; he also felt an odd sensation of dryness in and violent girding a-crofs the *thorax*. In less than half an hour he began to falter in his speech, became insensible, restless, and muttered frequently ; in which condition I found him. His extremities, and also the trunk of his body, were cold. His pulse small and quick. He often raised himself on his knees, continually stretched out his arms, and employed his hands as if searching for something he wanted ; his eyes were dull and heavy ; after some time, he became dumb and more quiet, had almost no pulse ; and, upon his being taken out of bed that it might be put into better order, his limbs were visibly paralytic. Altho' he changed postures a little, yet he remained stupid for six or seven hours ; then he raged furiously, requiring two persons to hold him in bed, notwithstanding which, he raised himself up, tossed greatly, and

and seemed to catch at the bystanders with his hands, uttering incoherent sentences. At last he became sensible and more quiet, restless and delirious by turns; and about ten o'clock in the evening of the same day perfectly recovered. After taking a purgative, he slept well all night, and had several stools in the morning. For the space of fourteen hours he neither slept, vomited, nor discharged any thing by stool or urine, tho' he frequently passes urine at other times, being grievously afflicted with the gravel.

ART. XII.

The Effect of Musk in curing the Gout in the Stomach ; by JAMES PRINGLE Esquire, late Surgeon to the third Regiment of Foot-Guards.*

A GENTLEWOMAN, aged 43 years, naturally of a delicate constitution, who has been for several years subject to hysteric fits, attended with a dry asthma, which her shape much contributed to ; was frequently attack'd to a violent degree with the gout in her head and stomach, as well as in all her extremities ; and with which she was lame the most part of Summer 1745. On the 3d of *November* following, she was violently seized with it in her stomach, which occasioned violent hiccups and convulsions of the part. The description she gave of it was, that as soon as these fits seized her, there came on a violent working of her stomach, and so great an agitation of her back, that her Maid was not able to keep her hand on it. By degrees

* Aprile 3. 1746.

degrees it rose to her throat, when she was almost strangled. She could by no means lie down, but was forced to sit night and day in an easy chair; and even then if she lean'd her head to the one side or other, it gave her great pain; so that she was obliged to sit in an erect posture. Her legs were very much swelled, which subsided a little on laying them on a chair; but as soon as that happened, the asthma returned. She did nothing all this while but keep herself warm, now and then drinking a little of some generous wine (as she said, to keep it out of her stomach); and once or twice took a little of the *tinct. sacra*. On the 21st of *November* about 9 o'clock at night, a Lady of her acquaintance, who had seen her in this condition, desired me to visit her, tho' she doubted if I should find her alive. Accordingly I went, and as I had seen such extraordinary effects of the *tonquin* medicine in the *singultus*, and had heard from Mr. *Read* of its efficacy in other nervous cases, I imagined it might be of some service here: and therefore I sent her the following bolus.

Rx *Cinnab.*

Rx *Cinnab. nativ.*

————— *Antimon. aua gr. xxv.*

Mosch. opt. gr. xvi.

Syr. bals. q. s. F. bolus.

BUT altho' this is Mr. *Read's* common dose, yet, as she was very weak, I ordered her to take only the one half of it immediately, drinking after it a cup of brandy, and the other half in six hours after. Next morning I found her much better, having from the first dose no more convulsions in her stomach. I then ventured to give her a whole bolus at 9 o'clock in the morning, to be repeated every four hours until such time as she should sleep or sweat: and notwithstanding the coldness of the weather, and her being obliged to sit in a chair, yet, by the time she had taken four boluses, a plentiful sweat and sleep ensued, and then she was able to lie in a horizontal posture on her couch without the return of her former symptoms. This sweat continued from the afternoon of the 22d till the 24th at night, with very little intermission. I gave over the boluses and ordered her a julep, to eight ounces of which I put twelve grains of musk,

to

to be taken *ad libitum*. In this method she continued till the 27th, quite easy and free from all her former symptoms, and even the swellings of her legs almost gone: But as on this day she fancied the gout in her stomach was returning, I gave her another bolus. She complained this time of the intolerable heat of the brandy, which was the first thing she had found warm in her stomach during this illness. On the 29th, she was apprehensive of another attack, and took another bolus; after which she found herself very well, and walked about the room, the swellings of her feet being quite gone. And on the 4th of *December* went out in a chair to thank the Lady who sent me to her, and continues to be well to this day.

ART.

ART. XIII.

An Account of an uncommon Effect of antimonial wine ; by Dr. JAMES WALKER Surgeon and Agent for the Navy at Edinburgh.*

THE *vinum benedictum* is universally known to be a strong emetic, when given in a large dose ; and it has also been often prescribed in smaller quantities as an attenuant, sudorific, and diuretic : but an accident lately discovered to me a very different effect of it, from any of the above mentioned.

BEING one evening, in *December* 1755, a little hot and feverish, with a quick and full pulse, I went early to bed, and drank a full *English* pint of sack-whey which I had ordered one to make for me. Very soon after this, I fell asleep, and continued all night oppressed with an unusual drowsiness : at ten next morning, I with great difficulty got so far the better of this lethargic disposition, as
to

* February 5. 1756.

to get out of bed ; when I found myself still heavy and inclined to sleep, with a lassitude and numbness in my limbs, so that I could scarce stand. Being thus incapable to go about business, I resolved to ride out on horse-back ; and having mounted with some difficulty, found my head very giddy, and had much ado to keep myself from falling asleep. After having rode three hours, I returned home much in the same situation, and was surprized to find two apprentices with the same complaints I had myself: they could assign no cause of their being so affected, nor had they eat any thing but their usual food, except the curd of which I had got the whey. Being led by this to suspect something uncommon in the wine with which the posset was made ; I called for it, and had a bottle brought me containing *vinum benedictum*, which had been made about a month before ; and the bottle having been put in an improper place, was the reason of its having been mistaken for *Lisbon*. I found I had drank in my whey about a gill and a half of this emetic wine, and was surprized it had not vomited me strongly. Thinking, however, that the finely attenuated particles of the antimony might

might be mostly suspended in the curd, I inquired particularly at the two young Gentlemen who had eat it, whether they were affected with any *nausea* or vomiting; but they told me they found no other effect but a heaviness and great inclination to sleep. This drowsiness continued with them two days; but I did not find myself quite free of it at the end of four days. I do not know if it be worth while to observe, that the day after I had drank the above whey, my pulse was ten in the minute slower than it had been the night before. It may be proper however to take notice, that the *vinum benedictum* which was used, by mistake, instead of *Lisbon*, was made exactly as is directed in the *Edinburgh Dispensatory*; and I have since found half an ounce of it vomit a patient very well.

ART.

ART. XIV.

An obstinate Dysentery cured by Lime-Water ;
 by JAMES GRAINGER M. D. Physician at
 London *.

— *Arfsdale*, a stout, middle aged, but intemperate soldier, was seized November 1751 with shivering and other symptoms that precede an acute disorder. The day following, he went frequently to stool, and evacuated blood to the quantity of a gill every quarter of an hour. Tho' the fever was inconsiderable, yet, as he was of a sanguine habit, the lancet was not spared. He swallowed some ipecacuan vomits ; was gently purged every third day with rhubarb ; and had small quantities of *opium* at bed-time to hinder his rising in the night. The third week his fundament came down, attended with but little pain, unless when he went to stool. This symptom, however, was timely removed by fomentations of a decoction of oak-bark. *December* ; his stools, tho' less frequent, were mixed with blood and *mucus*. Then

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* May 3. 1753.

the gripes seized him in good earnest, sometimes fixed and torturing like a stitch in the bastard ribs, at other times wandering with *borborygmi*, now as it were twisting his guts, then cutting him in two, as he expressed it, and bending him forward. They were always most severe before stool, easier in the time of evacuation, but easiest after. With these symptoms were complicated a dysury and piles, greatly inflamed, but bleeding none. These dismal complaints continued almost equally violent for three months, altho' *V.S.* emetics, *vit. antimon. cerat.* mild antiphlogistic purgatives, opiates, lubricating and astringent remedies were used, as the symptoms indicated. Opiates, tho' they eased him, seldom procured undisturbed repose, and always affected his head; this consequence of *laud.* was more effectually prevented by *bals. locatel.* than any thing else. Vomits always relieved him, but increased the pain of the hæmorrhoids. Astringents, tho' they bound him up for a little time, most generally brought on afterwards a more frequent inclination to stool, with increased *tormina*. Clysters were impracticable on account of the piles, but he found considerable service from
the

the milder purgatives, with calomel; and when the swelling of his fundament was removed by *sulphur* and emollient, discutient applications, they became highly useful. In the 4th month, the region of the stomach swelled, with redness of his face, especially after food, and continued for six months, altho' its removal was attempted by bitters, and the bark (cautiously combined with purgatives), mulled claret, and camomile tea, while proper external applications were not forgotten. *April* and *May*, the hæmorrhoids and dysury left him; but his stools became thinner, more acrid, and intollerably fetid. Then he was seized with a hiccup, his face looked ghastly, his extremities turned cold, his pulse became quick, small, irregular, and his gripes were rather abated. Evacuations in these deplorable circumstances were improper, but sinapisms were applied to the soles of his feet; and epispastics to the region of the stomach with some success. A bolus of bark, castor, and camphire was given every third hour, and washed down with a glass of mulled claret; his guts too were fomented with anodyne, emollient, and antiseptic clysters. By these his deadly symptoms left him,

him, he could sit up at the fire, and had not above 12 stools in the 24 hours, which were however still ichorous. I could not find from the nurse that he ever passed any of the villous coat of the guts, tho' *scybalæ* were frequently evacuated. About the end of *May*, he was able to walk round the ramparts of *Fort-William*, and he told he thought he would recover, were he sent from *Lochaber*. On this, he was carried, by water, to the Isle of *Mull*, being provided with proper medicines to forward his recovery. Here, tho' the hiccup and *facies hippocratica* did not recur, yet his gripes did; and he purged blood and worms almost incessantly. The latter end of *July*, he was sent back to the Fort a perfect skeleton; where, tho' I am convinced the air is inferior to that of *Castle-Dewart* in *Mull*, yet, as great care was taken both of his diet and medicines, he passed no more worms, his gripes only seized him at stool, whither he went much more seldom than formerly; what he passed now appeared to be *mucus* mixed with *pus* and streaks of blood. Very small doses of ipecacuan, *viz.* eight grains three times a day were then administered to him every third day; but

but, tho' they relieved by operating both up and down, yet they sickened him so much, that I was obliged to substitute a bolus of calomel. over night, and a purging ptisan next morning, in their place. The disease, however, did not yield ; and when the regiment was ordered to march for *Berwick upon Tweed*, he was put aboard the vessel that carried our baggage. This was a hardy step, considering his weakness, and the length and danger of the voyage ; but he could not be carried with his comrades over the *Black* mountain, and he desired to die any where, rather than remain in *Fort-William*. Five or six weeks after he landed at *Berwick*, his stools were more numerous, and still very painful. Sometimes blood, sometimes slime, sometimes ichor, and once he passed a great quantity of hardened excrements, which relieved him of a dull pain of his left hypochondre ; his legs too, at night, swelled and pitted to the touch, and his stomach was often inflated. Bitters, with steel, were prescribed, and camomile tea drank for breakfast, while the utmost regard was had to diet ; the *vit. antimon.* was again tried, and alum posset recommended. The
dysentery

dysentery baffled all our attempts; and now, despairing of his recovery, I ordered him to drink lime-water, with a third part milk, to the quantity of an *English* pint and a half every day: It was at the latter end of *November* 1752. In three days time, there was a sensible change to the better, his stools were less frequent, and his pains abated. Encouraged by this happy beginning, he was ordered to drink *lib. iii.* of lime-water a-day: This, in three weeks, made him so costive, that I was obliged to clyster him, and diminish the *aq. calc.* to the quantity first ordered. In six weeks from his taking this medicine, he was so thoroughly recovered, that he was dismissed the hospital, and soon after marched to his company at *Carlisle*, where he still enjoys perfect health. The disorder was on him full 14 months; and I have reason to think his recovery was chiefly owing to the lime-water, after the most celebrated antidyenterics had been used in vain.

THE dysentery is endemic at *Maryburgh*, near *Fort-William*, and commonly attended with *procedentia ani*, piles, dysury, abdominal inflations, *oidema's*, and hiccup. Many of the soldiers died, especially the more intemperate;

temperate ; between the 20th and 35th day, is the fatal period. Those who died had mortifications of the great guts : it began with us about the latter end of *October* ; autumn however is its usual season ; it rages commonly two months, tho' many have it all the winter, and I always observed them worst in rainy weather. It may be worth while to observe, that of late lime-water principally conduced to cure an Officer of a dysentery, while another was effectually cured of a weakness in the bladder, by the same remedy.

ART.

ART. XV.

The anthelmintic Virtue of the Bark of the wild Cabbage or Bulge-water Tree; by the late Mr. PETER DUGUID late Surgeon in Jamaica, in a Letter to ALEXANDER MONRO Senior, M. D. & P. A.

THE Writers on the diseases of the *West-Indies* generally take little notice of the Inhabitants of *Jamaica*, young and old, white and black, being much troubled with worms, especially the long round sort. They are however so frequent, that every practitioner ought to have regard to them in treating most of his patients. I imagine these worms infest the inhabitants here on account of their sweet viscid bread-kind, to wit, plantains, yams, bananos, sweetish potatoes, &c. which are fit nourishment for these vermin. I was lately allowed to open a Gentleman's child, that, at seven months of age, died of vomiting and convulsions. In its intestines there were twelve large worms: one of them

* May 1. 1755.

them filled the *appendix vermiformis*, and three of them were intertwisted in such a manner as to block up the opening at the *valvula Tulpii*, so that nothing could pass from the small to the great guts.

NATURE has bountifully provided the people here with a powerful remedy against so great an evil. This is the bark of a tree growing plentifully in this Island. The inhabitants call it *wild cabbage* or *bulge-water*: and from what I have seen, it appears to be the most powerful vermifuge yet known; for it frequently brings away as many worms by stool as would fill a large hat. It is commonly given in decoction, but not in any regular quantity; the negroes being generally the preparers of this medicine, and therefore no wonder that it sometimes has very violent effects. I am now making experiments for ascertaining the dose to patients of different ages, and shall soon send you the result of my trials, together with some of the bark itself.

ART. XVI.

The description of a monstrous Fœtus ; by Mr. JOHN MOWAT Surgeon at Langholm, in a Letter to ALEXANDER MONRO senior, M. D. & P. A.*

SEVERAL learned men having of late years disputed about the formation of monsters, it is probable the histories of them may be of use in accounting for some *phænomena* in nature ; on this account, I send you the following description of one, which will be better understood by the figures which young Professor *Monro* caused to be drawn of it, of half the natural dimensions. See plate VI. *fig. I.*

—————when six months with child, had this abortion, which lived half an hour.

TAB. VI. *fig. I.* A A The lower parts of the bodies of two female *fœtuses*, with their lower extremities in a natural state.

B The navel-string common to both *fœtuses*.

C The bodies joined immediately above the navel.

DD The

* May 1. 1755.

D D The superior extremities of the more compleat *fætus*, which is here represented most in view.

d d The superior extremities of the other *fætus*. All the four being of a natural form.

E The *sternum* of the more compleat *fætus* which had clavicles joined to it in the common way, as the other also had.

F One neck common in appearance to both *fætuses*.

The face and ears, being natural, need no letters of reference to their parts.

G The forehead had hair farther down than ordinary.

H The top of the head of an extraordinary breadth.

I The hairy scalp covering the parietal bones.

K The right temple of the *fætus* D D.

L The teguments covering the occipital bone.

M The *occiput* of the other *fætus*.

Fig. 2. The back view of these *fætuses* where the head, neck, and fore parts of the *thorax* only are represented.

N N The scalp over the four parietal bones.

OO ——— The two occipital bones.

PP Two ears joined together.

QQ The two *meatus auditorii*.

R A passage by which a probe could be put into the *œsophagus*.

S The neck of the *fœtus* D D.

T The *sternum*.

UU The forepart of the arms.

THE back parts of neither *fœtus* are drawn, having nothing preternatural. Where the *abdomen* of each *fœtus* was distinct at AA, the *viscera* were in a natural state, and in the undivided cavity of the belly above C, there were two stomachs, livers, spleens, *intestina duodena*, *jejuna* and *ilium*; but I saw only one *omentum* and one *pancreas*. One diaphragm divided the *abdomen* from the *thorax*, but it was pierced by two *venæ cavæ* and two *œsophagi*, and two *aortæ descendentes* passed between its *appendices* on each side where the *vertebræ* were.

THERE was a *sternum* to the *thorax* of both sides E fig. 1. and T fig. 2. From each *sternum* a *mediastinum* was extended to a ligamentous fleshy substance, which was continued transversely from one spine to the other. In each

each *mediastinum* was a *pericardium* with its heart and vessels as in natural *fætuses*, and on each side of each *mediastinum* was a *thorax* with lungs. Those in the *thorax* of the more compleat child, the one, to wit, with the face, being of a pale colour and swimming when put into water, while the other lungs were of a redder colour and sank in water.

THE spines gradually approached each other as they extended towards the neck, at the lower part of which the sides of the bodies of the *vertebræ* seemed to be contiguous; but I pushed a probe up between them as far as the second *vertebra*.

WITHIN the head I saw a *cerebellum* on each side divided from a common *cerebrum* by a membranous *plexus* produced from the *dura mater*. The parts of the *encephalon* were too soft and tender for my dissection, and I examined the anatomy no further, but have the monster in spirits with the three great cavities filled with lint and bran where the bowels were taken out.

ART. XVII.

The Dissection of the same Monster continued ;
 by ALEXANDER MONRO junior, M. D. and
 Professor of Anatomy in the University of
 Edinburgh*.

MY father having shewed me Mr. Mowat's description of the monstrous *fœtus*, and a model of its form in wax, I was curious to know how the parts of the head and neck were formed. The preserved *fœtus* was obtained with difficulty, and under a promise that no dissection should be made which might spoil its form. The subject was unfavourable, and the promise rendered the dissection difficult, and impossible to be performed so accurately as I wished.

AFTER drawing asunder the two parietal bones of each side I N N (see Mr. Mowat's *fig.*) to take out the lint, &c. with which the skull was filled, and having cleaned away the matter, membranes, &c. adhering to the bones, I saw the *cranium* of a natural

* May 1. 1755.

ral form on that side where the face is, so far back as the *fella turcica*, behind which the ends of the cuneiform processes of the two occipital bones united together. From each of these cuneiform processes the occipital bones extended of a natural enough form to each side; their situation may be judged by considering the figures at L, M, or O, O, having each a *foramen magnum* for the spinal marrow. At the side of each of the occipital bones, nearest to the conjoined preternatural ears, an *os petrosum* was placed, but without having any squamous part of the temporal bones, such as were on the other side of each occipital. Between these *ossa petrosa* there was a triangular little bone which sustained these preternatural ears, and was instead of *os ethmoides*, *sphenoides*, and two squamous bones. The *fætus* with the face had therefore all the common nerves, but the *fætus* with only the conjoined ears wanted the 1, 2, 3, 4, 5, and 6 pairs.

THE *meatus* Q, Q *fig.* 2. led into the organ of hearing. The orifice R was the entry to a passage which opened into the *æsophagus* of the *fætus* D D.

THE

THE mouth of the more compleat *fœtus* had all the ordinary parts, with the *uvula*, *nares*, *larynx* and *pharynx*. Behind this *pharynx* and the *œsophagus* descending from it, there was another *larynx* and *trachea*. From the back part of the *glottis* of this last mentioned *larynx*, a little excrescence resembling a tongue stood out, and behind it a canal descended of the form of one of the *nares* which joined with the one continued from R. to form another *œsophagus*.

I could not prosecute the vessels and nerves placed on the neck and head without breach of promise, and therefore can give no account of them.

ART.

ART. XVIII.

*Bones found in the ovarium of a Woman ; by Dr. GEORGE YOUNG, and communicated to the Society by Dr. JOHN BOSWELL, Fellow of the Royal College of Physicians in Edinburgh *.*

A Woman near fifty years of age, who had never had a child, being four months obstructed, thought she had conceived ; but the *menfes* then returning, she had excessive flooding, which was sometimes in great quantity, at other times was less, but scarce ever intermitted for a year and a half. It was then stopped by some medicine ; after which, her belly swelled to such a degree in six weeks, that her urine was almost totally suppressed ; she was very constive unless when clysters were given ; and she died in a few days more.

ON cutting the teguments of the *abdomen*, a large quantity of bloody water rushed out ; and when the containing parts were fully

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opened,

* November 1. 1737.

opened, all the cavity was bloody, and the vessels were large and turgid with dark-coloured blood. No bowel was now seen ; all that appeared in view being a great number of irregular fleshy lumps, which were bladders full of a red watery liquor. Some of them were of the size of the largest apples I have seen, others were as small as pigeon-eggs ; and there were of all the intermediate sizes between these. Upon a stricter examination, these vesicles were found to be all contained in one common cyst, of which I had cut the fore-part with the teguments of the *abdomen*. The large cyst filled all the belly : when it was raised, the bowels appeared in a natural state ; except that,

1. The left Fallopian tube was very large.
2. No *ovarium* of that side could be seen unless the great cyst was that *ovarium* immensely distended.
3. The right *ovarium* was as big as the head of a new-born child. It contained a viscid white-coloured substance resembling mashed brains, which run together like suet when put into water. In this stuff I found the bones herewith sent.

THE bone represented in *plate VI. fig. 3.* is a piece of a jaw with three firm *dentes molares*, A, in it. *Fig. 4. and 5.* are two views of a part of a jaw, in which are three *dentes molares*, B, irregularly set, and an *incisor C.*

ART.

ART. XIX.
*Proofs of the Contiguity of the Lungs and
 Pleura; by ALEXANDER MONRO Senior,
 M. D. and P. A.**

THE experiment of opening the *thorax* without hurting the lungs of living animals, while the trunks of their bodies are immersed in water, thro' which no bubbles of air rise after the perforation, as proposed by the ingenious *Lieberkühn* and executed by the illustrious *Haller* †, is a decisive one, if rightly performed, for proving no air between the *pleura* and lungs. But, as an unwary operator may wound the lungs in perforating the *thorax*, when air would certainly rise in the water, from which the existence of air in the *thorax* might be concluded, and several other circumstances may; and have caused the conclusion from this experiment to be disputed; it may not be amiss to mention some easier ways of proving

* February 7. 1754.

† Opusc. de respirat.

Fig. 1.



Fig. 2.

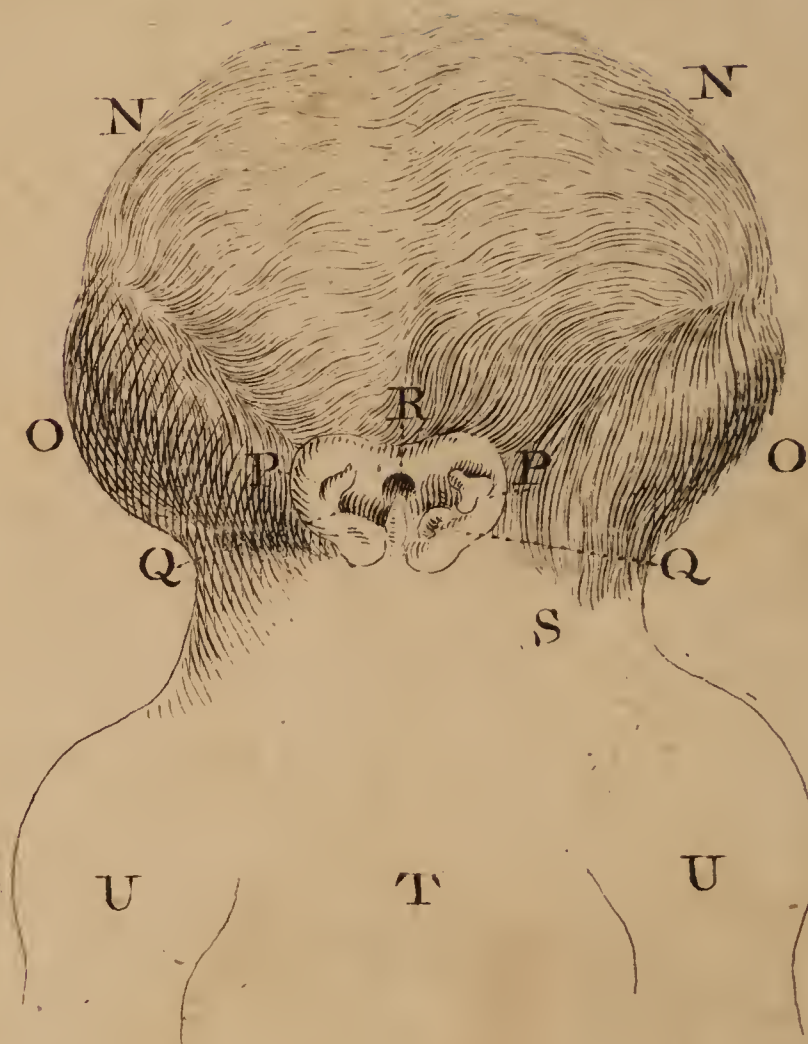


Fig. 4.



Fig. 5.



Fig. 3.



proving the non-existence of air in this place. Such are the following :

1. DISSECT the teguments and intercostal muscles from the *pleura* of either a dead man or quadruped without wounding this membrane, in which there is no difficulty ; then pull up and depress alternately the *sternum* and ribs as often as you will, the lungs are seen contiguous all the while to the *pleura* ; but, on making a small puncture thro' this membrane, the lungs, if they are not grown to the *pleura*, which is often the case in the human subject, fly from the *pleura*, and are no more seen.

2. THIS connection of the lungs and *pleura*, more or less of which is seen in most human bodies, implies strongly a natural contiguity of these two parts.

3. LAY bare the *pleura*, without wounding it, between two ribs of any living quadruped, which requires no great dexterity ; and then the contiguity of the lungs and *pleura* may be seen, tho' the lungs are constantly sliding and changing place along the *pleura*, and tho' this membrane is in different states : while the creature inspires, it is concave ;
during

during exspiration it is convex and prominent outwards; for this plain reason, that while inspiration is performing, the air does not pass so quickly at the narrow *glottis* as to fill the lungs at once with air of density and weight equal to the atmosphere; and during exspiration, the air cannot escape so fast at the *glottis* as to prevent its more than ordinary condensation and expansibility in the lungs than the external air has.

IF we were to find accurately what weight the *pleura* could raise when it is made convex during exspiration in the preceding experiment; would not this determine how much more pressure, than that of the atmosphere, the part of the lungs within this elevated *pleura*, on which this weight is sustained, is exposed to?

WOULD not the pressure on the same part of the lungs during inspiration be nearly as much less than the weight of the atmosphere, as is the weight raised in exspiration; since it is the same *glottis* which allows the air to pass in both cases?

Is not the force, by which the inspiratory organs, acting with the greatest energy of the mind, exceed the power of the expiratory

tory organs uninfluenced by the mind, considerably less than the weight of the atmosphere ; since, if, after expiration, we prevent the entry of the air by the *glottis*, we cannot dilate the *thorax* as in inspiration, tho' there is still a good deal of rarefying air in the lungs ?

Do not the inspiratory organs, during inspiration, overcome the resistance of the expiratory organs, and likewise that share of the pressure of the atmosphere, which the air, rushing into the lungs, does not balance ?

ARE not the ribs arched, and the spaces between them narrow, to prevent ill effects from that unbalanced part of the atmosphere during inspiration ?

ART.

ART. XX.

An Account of some Experiments made with Opium on Living and Dying Animals;
 by ROBERT WHYTT M.D. F.R.S.
*Fellow of the Royal College of Physicians,
 and Professor of Medicine in the University
 of Edinburgh*.*

THE ancient physicians imagined that *opium* extinguished the flame of life in animals by its excessive cold; and in later times, there have not been wanting those who deduced its effects from a quite opposite quality, whereby it was thought to rarefy the blood and to compress the brain or origin of the nerves. These false notions, however, of the nature and action of *opium*, have been refuted by several of the moderns, whose writings have thrown considerable light upon this subject.

THE following experiments were made with a view still further to illustrate the manner in which this wonderful drug produces
 its

* August 7. 1755.

its effects, and particularly to shew its influence upon the motion of the heart.

1. HAVING injected a solution of *opium* in water into the stomach and guts of a frog, I observed, that in little more than half an hour it seemed to have lost all power of motion, as well as feeling; for there was no contraction produced in the muscles of its limbs and trunk by irritating them. I opened the *thorax* an hour after the injection, and found the heart, instead of between 60 and 70, making only 17 pulsations in a minute. The auricle, which was much distended with blood, always contracted first, and after it the ventricle.

2. A frog continued to move its limbs, and leap about for above an hour after I had cut out its heart, and was not quite dead after two hours and a half.

FIVE minutes after taking out the heart of another frog, I injected a solution of *opium* into its stomach and guts. In less than half an hour, it seemed to be quite dead; for neither pricking nor tearing its muscles produced any contraction in them, or any motion in the members to which they be-

longed. After cutting off its head, a probe pushed into the spinal marrow, made its fore-legs contract feebly.

3. EIGHTEEN minutes past four in the afternoon, I injected a stronger turbid solution of *opium* in water than that used in the preceeding experiments*, into the stomach and guts of a frog; and as it squirted out most of the solution injected by the *anus*, I threw in some more in its place. At twenty four minutes past five, I opened this frog, and observed the heart with its auricle greatly distended with blood and beating very slowly, not above seven times in a minute. When the heart was touched with the point of a pair of scissars, its motion was rendered quicker for two or three pulsations: after which it became as slow as before.

4. IMMEDIATELY after decollating a frog, I destroyed its spinal marrow, by pushing a small probe down thro' its spine, which occasioned strong convulsions of all
the

* *Viz.* half an ounce of *opium* dissolved in eight ounces of water; which was also made use of in all the following experiments. The heat of the solution was nearly the same in all the experiments; *viz.* about 60 degrees of *Fahrenheit's* thermometer.

the muscles, especially those of the inferior extremities. Ten minutes after this, I opened the *thorax*, and found the heart beating at the rate of 45 times in a minute. Sixteen minutes after decollation, it moved 40 times in a minute. After half an hour it made 36, and after fifty minutes only 30 pulsations in the minute, which were now also become very small and feeble.

N. B. WHEN I opened the *thorax* of another frog immediately after decollation, and destroying its spinal marrow, I observed its heart beating at the rate of 60 in a minute, which is four or five pulsations less than I have generally seen the hearts of frogs make in that time, when their *thorax* was opened without decollation.

5. AT nine minutes past eleven in the forenoon, immediately after decollating another frog, I destroyed its spinal marrow with a red hot wire, which produced terrible convulsions in all the muscles, as in the last experiment. I opened the *thorax* of this frog *thirty five minutes* after decollation, and observed its heart beating 30 times in a minute. The contraction of the auricle regularly preceded that of the heart: the
auricle

auricle was not near so much distended with blood, nor the heart so much swelled as in those frogs which had a solution of *opium* injected into their stomach and guts*. At one o'clock (*viz.* an hour and fifty one minutes after decollation), the heart of this frog made 20 pulsations in a minute. At half an hour past two, when the room was become warmer by the shining of the sun, it beat 25 times in a minute; and when placed in the sun-beams, it performed 31 contractions in that time. After this, I removed the frog to an east window, where it was exposed to a cool breeze; upon which the motion of its heart became slower, so that in a short time it only made 25 pulses in a minute. I then exposed it anew to the sun-beams, by which its motion was soon quickened, so that it beat 30 times in a minute.

At twenty five minutes past five in the evening, (*viz.* six hours and sixteen minutes after decollation and the destruction of its spinal marrow) the auricle of this frog's heart, which was still filled with blood, contracted

* See No. 3. above, and Essay on the Vital and other Involuntary Motions of Animals, p. 371 & 372.

ed twelve times in a minute ; but the heart itself lay without motion, was swelled and very red : however, when pricked with a pin, it performed two or three pulsations, and then remained at rest, till roused by a new *stimulus*. At thirty five minutes past five, the heart seemed to be quite dead, but the auricle continued its motion ; nay, at half an hour past eight, near three hours after the heart had been without motion, the auricle, which was very near as much filled with blood as when I first opened this frog, beat 11 or 12 times in the minute ; its pulsations, however, were not now so regular as to time, as they had been before.

Is it not probable, that the auricle of this frog's heart beat longer than usual, because it continued, to the last, to be filled with blood ; whereas, generally, the auricles of frogs hearts, which are opened after decoliation and the destruction of their spinal marrow, expell, after some time, the blood which they contain, and acquire the appearance of a small pellucid bladder filled with air ?

6. I laid bare the abdominal muscles and *thorax* of a frog, by dissecting off the skin, and

and at twenty minutes before nine in the morning, I immersed the whole body of the frog in a turbid solution of *opium* in water, in a small bason, which I covered, to prevent the frog from leaping out of it. *Thirty five minutes* after immersion, I took it out of the solution and opened the *thorax* and *pericardium*. The heart's auricle, which was much distended with blood, beat 15 times in a minute, but the heart itself, only 6 times. Forty minutes past nine (*viz.* twenty five minutes after the frog was taken out of the solution of *opium*) the heart seemed to have recovered more life; for it performed eight pulsations in a minute: the contractions of the auricle now became feebler, and were scarce more numerous than those of the heart, but always preceeded them some little time. Six minutes before ten this heart moved only six times in the minute. Twenty four minutes past ten it made only five pulsations in sixty five seconds, the first, third, and fifth of which pulsations were after an interval of fifteen seconds, and the second and fourth after a pause of ten seconds. Seventeen minutes before twelve, and *two hours and twenty eight minutes* after the
frog

frog was taken out of the solution of *opium*, its heart moved only thrice in seventy five seconds, and performed its *systole* very slowly. Before two o'clock afternoon the heart was quite dead; but how long, I cannot say, not having had leisure to observe it from a quarter before twelve to near two.

7. AFTER cutting off a frog's head and destroying its spinal marrow with a red hot wire, I laid bare the abdominal muscles and *thorax*, as in the last experiment, and immersed the whole body of the frog in a turbid solution of *opium*, at half an hour past nine in the morning. *Thirty six minutes* after immersion I took it out of the solution, and opened its *thorax* and *pericardium*. The heart and its auricle beat, each, twenty six times in a minute, and the pulsations of the auricle preceeded those of the heart regularly. The heart did not appear to be more swelled or redder than in a natural state, and the auricle was not near so full of blood as in *Exp. 6*. Twelve minutes past ten, *viz.* six minutes, after this frog was taken out of the solution of *opium*, its heart beat 27 times in a minute. At eleven o'clock it performed 18 vibrations in that time; and 16 at a quarter

quater before twelve. At two o'clock afternoon, the auricle, which, having expelled all its blood, was now only filled with air, continued its motions ; but the heart lay at rest. Ten minutes past four, *i. e.* five hours and forty four minutes after the frog was taken out of the solution, the auricle of its heart beat 9 times in sixty four seconds.

8. I laid bare the abdominal muscles and *thorax* of another frog, and at fourteen minutes past eight in the morning, immersed it as above in a turbid solution of *opium*. Fourteen minutes past nine, I took it out of the solution, and laid open its *sternum* and *pericardium* ; after which the heart began to beat at the rate of nine times in a minute : but the auricle, which was greatly distended with blood, made no motion, except in so far as it was agitated a little by the pulsation of the heart : nor were the muscles of the legs or thighs brought into contraction by cutting or tearing their fibres. At half an hour past nine the heart beat only 7 times in a minute ; and the auricle, which was now pretty empty of blood, and, in place of it, filled with air, had a pulsation as well

as the heart. Thirteen minutes before ten, *i. e. thirty three minutes* after the frog was taken out of the solution, the auricle shewed, at considerable intervals, a very faint pulsation, but the heart lay without any motion.

9. THE same day, after cutting off the head and destroying the spinal marrow of another frog, I laid bare its abdominal muscles and *thorax*; and, at eighteen minutes past ten, immersed it in a solution of *opium*, as above. Eighteen minutes past eleven, I took it out of the solution and opened its *thorax* and *pericardium*, after which the heart began to move at the rate of 8 times in a minute. Twenty five minutes past eleven, the heart beat 15 times in a minute; and at twelve o'clock it performed between 13 and 14 vibrations in the same time. At two o'clock, (*viz. two hours and forty two minutes* after the frog was taken out of the solution) the auricle, which was now filled with air, continued to vibrate weakly, about 11 times in the minute; but the heart itself was without motion. At ten minutes before four in the afternoon, the auricle still continued to

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move, but more feebly than the auricle of N° 5.

10. I laid open the whole *abdomen* of a larger frog than any of the former ; and, at twenty two minutes past ten in the morning, immersed it in a solution of *opium*, as above. *Thirty five minutes* after immersion, I took it out of the solution, and opened its *thorax* and *pericardium*. The heart was vastly red and much swelled, and its auricle greatly distended with blood ; but both were without any motion : after two minutes, however, the heart began to vibrate at great leisure, scarcely performing nine pulsations in a minute ; but the overstretched auricle made not the smallest motion. During every *systole*, the heart was remarkably paler, and in the time of its relaxation became much redder ; which *phænomenon* I observed likewise in all the frogs hearts in the above experiments, but more remarkably in those frogs who had been exposed to the action of *opium*. Another thing, which I remarked in all these experiments, was, that the heart, during its *systole*, became manifestly shorter, and was lengthened in the time of its relaxation. But to return ; at six minutes
past

past twelve, (*i. e. an hour and nine minutes* after the frog was taken out of the solution) its heart made only 6 pulsations in the minute ; and at eleven minutes past twelve, observing it without motion, I pricked it with a pin, and breathed upon it, in order to renew its pulsation, but to no purpose.

II. TWENTY eight minutes past seven in the evening, I laid open the whole *abdomen* and *thorax* of a frog, and immediately after immersed it in a solution of *opium* as above. Thirty eight minutes past seven, when I pricked its legs with the point of a penknife, it made very little motion. Two minutes after this, I turned it to its back, and observed its heart moving only between ten and eleven times in a minute. Having laid the frog again on its belly, that it might be more exposed to the action of the *opium* ; at forty eight minutes past seven, *i. e. twenty minutes* from the first immersion, I turned it again to its back, and observing the heart without motion, I opened the *pericardium* ; which producing no effect, I cut the heart out of the body, and laid it on a plate, when it gave two or three pulses, and never after moved,

moved, tho' it was pricked once and again with a pin.

No motion was produced in any of the other muscles of this frog, by irritating them.

12. I cut off a frog's head and destroyed the spinal marrow with a hot wire, then laid open its *thorax* and *abdomen*, and immersed it in a solution of *opium* at nineteen minutes past eleven. Eight minutes before twelve, *i. e. thirty three minutes* after immersion, I observed its heart beating very slowly: but two minutes before twelve, when I took it out of the solution of *opium*, it had no motion. After, this I opened the *pericardium*, and irritated the heart two or three times with the point of a *scalpel*, which always produced a few pulsations. I then put the frog in the solution for five minutes more, and, upon taking it out, found its heart quite dead.

13. AFTER cutting off a frog's head and destroying its spinal marrow, I laid open its whole *abdomen*, and immersed it in a solution of *opium*, twenty three minutes before one. After it had lain *sixteen minutes*, I cut up its *thorax* and *pericardium*; and observing the heart

heart beating very regularly and pretty strongly, 21 times in the minute, I immersed it again in the solution, which had now immediate access to the heart. After five minutes, I took it out of the solution; and finding the heart without motion, I pricked it with the point of a knife; upon which it began to beat at the rate of 14 times in the minute, and continued its motions very languidly, and not without some interruption, for about a quarter of an hour.

14. I cut out the heart of a frog, and put it in fountain-water at ten minutes past ten; immediately after immersion, it beat about 28 times in the minute. Eighteen minutes past ten, it made 6 pulsations in thirty seconds. Twenty minutes after ten, I took it out of the water and laid it on a table, and observed, that as often as it was gently touched with any thing, it made one full and strong contraction, and no more: however, in four or five minutes, it began to beat of its own accord, and, at twenty eight minutes after ten, performed 19 pulsations in a minute. Thirty five minutes past ten, it beat 12 times in a minute,

15. TWENTY

15. TWENTY three minutes past twelve, I cut out the heart of another frog, and put it in fountain-water. After *twelve minutes* immersion, I took it out of the water, when it beat above 20 times in a minute. Having put it in the water for five minutes more, it ceased from motion, and when taken out, did not move except when pricked, and then only performed one pulsation.

16. EIGHT minutes past eleven, I cut out the heart of a third frog, and put it into fountain-water. Eleven minutes after immersion its heart beat 8 times in the minute, and four minutes after this it vibrated 11 times in thirty seconds; but the motion was confined to about one third part of the heart next its *apex*. *Twenty minutes* after immersion, it continued to move much in the same way; but in two minutes more, observing no motion in it, I took it out of the water, and laid it on a table, where it remained at rest, unless when touched. Soon after this, however, it began to move; and at twenty five minutes after immersion, it made 9 pulses in sixty three seconds. Four minutes after this, it moved only thrice in fifty seconds, and then ceased altogether; unless

unless that, when pricked with the point of a knife, it gave one very faint pulsation. At forty seven minutes past eleven, it was quite dead.

17. I cut out the heart of a fourth frog, and at thirty minutes past ten immersed it in a turbid solution of *opium* in water of the same degree of heat with the fountain-water used in the three last experiments*. After this heart had been immersed *ten minutes*, I took it out of the solution, and laid it on a table, but it made not the smallest motion; and when pricked with the point of a knife, tho' it quickly recovered its shape, yet it was not excited into a proper contraction, as the heart of N° 14. I continued to observe this heart from time to time for more than half an hour, but it never made the least motion.

18. I cut out the heart of a fifth frog, and put it into a solution of *opium* in water five minutes before eight. After *seven minutes* immersion, I took it out, and laid it on a plate, where it remained at rest. When pricked with a knife, it did not perform a full pulsation like N° 14. but seemed to feel

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* *Viz.* Nearly sixty degrees of *Fahrenheit's* thermometer.

a little, by a very faint kind of motion which was excited in some of its fibres.

19. AT thirteen minutes before twelve, I cut out the heart of a sixth frog, and immersed it in a solution of *opium*. Six minutes after immersion, it had no motion; but when pricked, made one pulsation. After lying five minutes more in the solution, it was quite dead.

20. I cut out the heart of a seventh frog, and at thirty seven minutes past nine in the morning, immersed it in a solution of *opium*, as above. Forty two minutes after nine, when I took it out of the solution, it was without motion: but when touched with the point of a knife or probe, it performed one contraction, but with less vigour and more slowly than the heart of N^o 14. Forty seven minutes past nine, it began to beat of its own accord. Two minutes after this, it moved 6 times in the minute, but much more feebly than N^o 14. Six minutes before ten, it beat only 4 times in a minute: after this, it began to beat much faster; but its motions soon returned to their former slowness. At ten, after having lain near a minute without motion, it began again, of
its

its own accord, to beat at the rate of 17 times in the minute, and continued for eight or ten minutes after this to beat very feebly, and in an irregular manner as to time.

21. MR. *Robert Ramsay* Student of medicine in this place, having dissolved two scruples of *opium* in an ounce of water and a dram of liquid *laudanum*, injected it blood-warm into the *intestinum rectum* of a very small dog near six months old. In less than a minute after the injection was made, the dog could not stand on his hinder legs; and in 3 or 4 minutes he had lost the use of them so much, that when they were strongly pinched, he neither moved them, nor seemed in the least degree sensible of pain. He could, however, still scramble about with his fore-legs; and when they or his ears were pinched, he howled remarkably, and seemed to feel considerable pain. Ten minutes after the injection, he lay as if he had been quite stupid; only when a noise was made by beating on the ground, he opened his eyes a little and howled, but presently after fell into a profound sleep. In a few minutes after this, he began to be convulsed; upon which Mr.

Ramsay injected a strong solution of sea salt in water into his guts, which purged him severely, and occasioned a *prolapsus ani* ; soon after this, he awaked from his sleep, and gradually recovered the use of his hinder legs; so that in less than an hour he could run about the room, tho' he often fell down, his legs bending under him. After three or four hours, he seemed to be quite well in every respect ; but altho' the experiment was made at mid-day, he could taste no meat till late at night. When he was in the most stupid state, he could make use of his fore-legs, and complained when his ears were pinched.

22. THE same young Gentleman, at my desire, made the following experiment. On the 9th of *April* 1755, after making an opening into the cavity of the *abdomen* of the dog on whom the last experiment was made, he injected by the wound a dram of *opium* dissolved in two ounces and a half of water ; but before he could stitch up the wound, about an ounce of the solution escaped. The dog lost the power of his hinder limbs almost instantaneously. Two minutes after the injection was made, he began to be convulsed.

fed ; and, in two minutes more, after having raised himself upon his fore-legs, he fell down senseless. At this time Mr. *Ramsay* laid bare the *thorax*, by dissecting off the teguments, which did not seem to give the dog any pain, and could plainly feel the motion of his heart thro' the *pleura* : it beat 76 times in a minute, but became gradually slower *. Immediately after counting the pulse, Mr. *Ramsay* cut the ribs on each side of the *sternum*, which he laid back in the usual way. The heart, which was thus brought in view, appeared quite turgid, and continued in motion about five minutes ; during which time it performed only between 60 and 65 weak vibrations, for they were not compleat contractions. While the heart was thus moving, warm *saliva* was first applied to it, then cold water, and last of all oil of vitriol ; which shrivelled the parts it touched, almost in the same manner as a hot iron would have done ; but none of them accelerated the heart's vibrations, which became gradually slower, till they ceased altogether.

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* The dog's heart in a natural state, and before the injection of the solution of *opium*, beat 150 in the minute.

THE fibres of some of the intercostal muscles on the right side of the *sternum* continued to be agitated with a weak tremulous motion near half an hour after the injection was made into the *abdomen*; but the intercostal muscles attached to the ribs on the sides of the *thorax* were not observed to move, nor did the diaphragm make any motion when its fibres were pricked or cut.

NOTHING remarkable was seen in the *abdomen*; only, altho' it was opened ten minutes after making the injection, the intestines had no motion; whereas, in another young dog, which had got no *opium*, Mr. Ramsay observed the peristaltic motion continue half an hour after laying open the *thorax*.

THE dog lost little or no blood in making the wound into his *abdomen*, nor were any of his bowels hurt by it.

23. A small dog into whose stomach the late celebrated Dr. Mead had forced, at four different times, a solution of two drams of *opium* in water, lived above an hour and three quarters after getting the first dose. *Vid. treatise on poisons, Essay IV.*

24. IT may not be improper to add here an experiment related by Dr. Alston in his
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learned dissertation on *opium**. Into the crural vein of an old dog forty two pounds weight, he caused be injected at three different times, half an ounce of *opium* dissolved in four ounces of water, filtrated, and of the same warmth with the blood of the animal. The first time, about fifteen drams were thrown in, and very slowly. It had no observable effect. About an hour after, eight drams more were injected slowly, and immediately the dog was seized with strong convulsions; the pulse was frequent and small, and after some time he foamed at the mouth. But there appearing no signs of immediate death, after waiting an hour more, the last nine drams were thrown in quickly; upon which the pulse became full and slow, and in a minute or so, the dog expired.

FROM the preceeding experiments, we may, I think, fairly draw the following conclusions.

(a) *Opium* applied to the stomach, guts, cavity of the *abdomen* and *thorax* and abdominal muscles, soon lessens, and after some time intirely destroys all feeling and power

* Edinburgh Med. Essays, vol. v. p. 1. art. xii.

of motion, not only in the parts to which it is applied, but thro' the whole body.

N^o 1, 2, 3, 8, 11 & 22.

(b) *Opium* produces these effects much more quickly in animals which are soon killed by want of food and air, than in those which can live long without them, and the parts of whose bodies preserve a power of motion and appearances of life for a considerable time after they are separated from each other. N^o 1, 3, &c. compared with N^o 21, 22 & 23.

(c) SINCE a solution of *opium* injected into the stomach and guts destroys the sensibility and moving power of frogs, fully as soon when they are deprived of their heart, as when this organ remains untouched; it follows, that *opium* applied to these parts, does not produce its effects by entering the blood, and being, by its means, conveyed to the brain, as some have imagined, but by its immediate action on the organs and parts which it touches. N^o 1. compared with N^o 2. See also *Edinburgh Medical Essays*, edit. 3. vol. 5. part 1. page 140.

(d) SINCE, after decollation and the destruction of the spinal marrow, *opium* operates

rates much more slowly in destroying the heart's motion in frogs, than it does when the animals are intire (N^o 6. compared with N^o 7.) ; it follows, that it must produce its effects chiefly, if not wholly, by its action on the brain, spinal marrow, and nervous system. The heart of the frog N^o 7. whose brain and spinal marrow had been destroyed, beat 27 times in a minute, after the animal had lain thirty six minutes in a solution of *opium* ; which was only three pulsations less than the heart of the frog N^o 5. performed thirty five minutes after the destruction of its brain and spinal marrow, altho' it was not exposed to the action of *opium*.

(e) WHEN *opium* injected into the veins, and thus mixed with the blood, lessens or destroys the sensibility and moving power of animals much in the same way as when it is applied to their stomach, guts, or cavity of the *abdomen* (N^o 24) ; is it not probable, that it produces these effects by its action on the extremities of the nerves which terminate upon the internal surface of the heart and whole vascular system ; and perhaps, also, by affecting immediately the *medulla cerebri* itself ? And when a solution of *opium* applied

applied to the bare abdominal muscles of a frog deprived of its brain and spinal marrow, does, after a long time, considerably impair the heart's motion; is it not reasonable to think, that this is owing to the finer parts of the *opium* being absorbed by the bibulous veins and carried to the heart, and thus brought into contact with the nerves of this organ? N^o 7. compared with N^o 9.

(f) SINCE *opium*, without entering the blood or being carried to the several parts of the body, destroys the power of feeling in animals *merely* by acting on the nerves to which it is applied (c) (d), it follows, that the nerves are the instruments of sensation, or, at least, necessary to it. Nor is it sufficient to destroy this conclusion, that there have been instances of animals endowed with feeling whose brains were so greatly diseased, as to *seem* incapable of performing their functions. It is far from being safe to build theories in physic upon a few monstrous appearances in nature.

(g) It appears from N^o 4. and 5. compared with N^o 3, 6, 8, 10 and 11. that decollation and the destruction of the spinal marrow does not weaken or destroy the
heart's

heart's motion in frogs, near so soon as *opium* injected into their stomach and guts, or applied to the muscles and bowels of the lower belly and *thorax*.

(b) ALTHO' a solution of *opium* applied to the opened *thorax* and *abdomen* of a frog, after decollation and the destruction of its spinal marrow, soon weakens or destroys the motion of the heart; yet it does not produce these effects so speedily as when the brain and spinal marrow are intire, N^o 11. and 12. In the former case, the *opium* can only affect the heart by its topical influence; in the latter, it not only acts this way, but also exerts its powers upon the brain, spinal marrow, and whole nervous system; and therefore must produce more sudden effects.

(i) It appears, beyond doubt, from the preceeding experiments, that the heart is not exempted from the power of *opium*, as the learned Dr. *Haller* has affirmed*, but has its motion destroyed by it, as well as the other muscles, only not so soon. See N^o 4. and 5. compared with N^o 3. 6. 8. & 10.

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* Act. Gotting. vol. ii. p. 147 & 154.

and N^o 14. 15. & 16. compared with N^o 17. 18. 19. & 20.

It is true, that the fibres of the intercostals on the right side of the *sternum* of the dog N^o 22. continued to be agitated with a tremulous motion considerably longer than the heart, and when the intercostal muscles attached to the ribs were quite dead. But did not this happen because, after separating the *sternum* from the ribs, and thus cutting off all communication between it and the spinal marrow, the muscles attached to it, could be no more affected by the *opium*, which had been injected into the cavity of the *abdomen*; while the heart and other muscles whose communication, by means of the nerves, with the brain and spinal marrow, was intire, continued to be exposed to its action?

(k) As Dr. *Langrish* has observed, that the distilled water of laurel-leaves injected into the cavity of the *abdomen*, kills dogs sooner than when it is taken into the stomach*; so N^o 21. and 23. compared with N^o 22. shew that *opium* injected into the stomach and

great

* Physical experiments on brutes, p. 64.

great guts of dogs, does not produce either such speedy or powerful effects, as when thrown into the cavity of the *abdomen*. And N° 6. compared with N° 10. shews, that a solution of *opium* applied to the abdominal muscles, does not kill frogs so soon as when all the *viscera* of the lower belly are exposed to its action.

(l) ALTHO' it seems probable, from N° 22. compared with N° 24. that a solution of *opium* injected into the veins of dogs does not kill them so soon as when thrown into the cavity of the *abdomen*; yet this cannot be certainly concluded, since the dog of N° 24. was much older and above ten times heavier than the other.

(m) IT appears, that a solution of *opium* injected into the great guts of a dog, affects the inferior part of the spinal marrow much more remarkably than its superior part, or the brain; since the dogs of N° 21. and 22. not only lost the power of motion sooner in their hinder legs than in their fore ones, but also were insensible of any pain in them, and yet howl'd strongly when their ears were pinched.

(n) A

(n) A solution of *opium* injected into the cavity of the *abdomen* or great guts of dogs, does not destroy the feeling and power of motion of their hinder limbs, by sending any *effluvia* to their muscles ; otherways it could not produce these effects so instantaneously, (N^o 21. & 22.) Besides, since *opium* thrown into the stomach and guts of a frog after being deprived of its heart, destroys the sensibility and moving power of its muscles equally soon, as if the animal had been intire (N^o 2.) ; 'tis plain, that these effects cannot be owing to the finer parts of the *opium* being received into the blood, and by its means carried to the several muscles and organs.

(o) NOR does a solution of *opium* injected into the great guts or cavity of the *abdomen* in dogs produce its effects by transmitting through the nerves any subtile *effluvia* to the spinal marrow ; otherways its operation could not have been so instantaneous (N^o 21. & 22.) ; nor could the spinal marrow and its nerves have recovered their functions so soon, after the *opium* was evacuated by a purgative clyster, N^o 21.

(p) IT

(p) It remains, therefore, that *opium*, by affecting the extremities of the nerves of the parts to which it is applied, does, by means of their connexion and sympathy with the brain and spinal marrow, destroy or prevent, through the whole nervous system, the operation of that *power* upon which depends sensation and motion in the bodies of animals.

(q) SINCE *opium* applied to the abdominal muscles of a frog, deprived of its brain and spinal marrow, does not destroy the motion of the heart so soon, as when it is applied to the abdominal muscles of a frog whose brain and spinal marrow are intire, (N^o 6. and 7.), it follows, that the brain and spinal marrow, and consequently the nerves derived from them, have a greater influence than any other part of the animal system, upon the motion of the heart

(r) OPIUM does not only destroy the moving power of the muscles of animals by intercepting the influence of the brain and spinal marrow, but also by unfitting the muscular fibres themselves, or the nervous power lodged in them for performing its office: otherways a solution of *opium*, when applied

applied to the abdominal muscles, or *viscera* of a frog, would not put a stop to the heart's motion sooner, or indeed so soon, as decol-lation and the destruction of its spinal marrow, (N^o 4. & 5. compared with N^o 8. & 10.). *Opium* therefore does not produce its effects, *solely*, by putting a stop to the function of the brain and spinal marrow, but its influence reaches to the fibres of the muscles themselves, or to the extremities of the nervous filaments which terminate in them.

WHEN I say the influence of *opium* reaches to the nervous filaments which terminate in the muscular fibres, it is not meant, that any *effluvia* or subtile parts of the *opium* are transmitted to them (See *n* & *o* above), but that it destroys their powers, by means of that sympathy which they have, through the brain or spinal marrow, with the nerves to which the *opium* is immediately applied.

(*f*) FROM the above experiments we may infer, that not only the power of voluntary motion in the muscles, but also their *irritability* or power of motion, when stimulated, proceeds from the nerves, or is at least immediately

mediately dependent on their influence; since *opium*, which produces its effects, *solely*, by affecting the nervous system (*m, n & o*), destroys those powers so suddenly. I know it has been lately argued by a celebrated author, that the irritability of the muscles must be independent of the nerves, because the muscles of animals preserve a power of moving when irritated, for some time after the communication between them and the brain, by means of the nerves, is cut off*. But since a solution of *opium* applied to the abdominal muscles of frogs, *merely* by its action on the nerves, puts a stop to the irritability or moving power of the heart, much sooner than the destruction of the brain and spinal marrow (*g*); is it not reasonable to conclude, that the tremulous motions of irritated muscles after their nerves are tied, proceed from the integrity of the nervous filaments below the ligature, and the nervous power still remaining in them or in the muscular fibres themselves?

THE tying or cutting of a nerve, *only* prevents the derivation of any new influence to it, but does not destroy the influence from

* Acla Gotting. vol. ii. p. 134, &c.

from the brain, to the parts to which it belongs ; but does not immediately destroy the *power* or *influence* remaining in the nerve itself. *Opium* applied in sufficient quantity to the sensible parts of animals, not only quickly puts a stop to the function of the brain and spinal marrow, and thus produces in the muscles all the effects of a ligature on their nerves, but also destroys the *power* of every nervous filament in the body (*r*), and therefore puts a stop to the motion of the heart in frogs sooner than the destruction of the brain and spinal marrow.

(*t*) THE almost instantaneous palsy brought on the hinder legs of a dog, by injecting a solution of *opium* into the cavity of its *abdomen* (N^o 22.), and the effects of the same solution injected into the stomach and guts of a frog deprived of its heart (N^o 2.), where no part of the *opium* could be conveyed to the muscles, nor be conceived to alter the nature of their *gluten* ; shew, that the *irritability* of the muscles has not its seat in this *glue*, as some have lately imagined*. But if the motions of irritated muscles be owing to a disagreeable sensation excited in them or their

* Aët. Gotting. vol. ii. p. 152.

their nerves, as we have elsewhere endeavoured to shew *, 'tis easy to see that *opium* must, by destroying the *sensibility* of the muscles, of consequence also destroy their *irritability*.

(u) IN animals which have got a large dose of *opium*, the veins, especially those of the membranes of the brain, are observed to be much swelled ; whence it has been thought, that *opium* produces its effects in the bodies of animals, partly, at least, by rarefying the blood and compressing the brain : but this distension of the veins seems to be no more than a consequence of the very slow motion of the blood through the heart, on account of the insensibility with which this organ is affected †.

(v) SINCE *opium* soon puts a stop to the vital motions of animals, which yet continue

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* Essay on the vital and other involuntary motions of animals, sect. ix. and Physiological Essays, p. 188, &c.

† In frogs, into whose stomach and guts I had injected a solution of *opium*, I not only found the heart's auricle, but also the great veins leading to it, much distended with blood. Vid. Essay on vital motions, &c. p. 371. and 372.

in time of sleep with little or no diminution of their vigour ; since it often eases pain without bringing on sleep, and since, by its topical action on the heart, it destroys the motion of this organ after all communication between it and the origin of the nerves is cut off* ; it follows, that the effects of *opium* are not owing, as some have thought, to its producing sleep : on the contrary, the sleep which it occasions, seems to be only a consequence of its impairing the sensibility of the whole nervous system.

THE other effects of *opium* may be also deduced from the same cause, particularly its restraining all evacuations that are owing to an unusual irritation of the parts of the body, and at the same time promoting those natural secretions which have been diminished or stopt by spasmodic strictures of the vessels, from some uncommon *stimulus* affecting them.

(w) LASTLY, does not *opium* kill animals by rendering their several organs wholly insensible of the *stimuli*, which are destined by nature to excite them into action ; whence

not

* Vid. N° 12. 13. 17. 18. 19. & 20. above.

not only a stop is put to the peristaltic motion of the guts, and to the propulsion of the chyle *, but the fluids also begin to stagnate first in the smaller and afterwards in the larger vessels † ; while the heart becoming gradually less sensible of the stimulus

* In a small dog, which Dr. *Kaau Boerhaave* opened, after having given him three grains of *opium*, he observed scarce any peristaltic motion in the guts: the stomach was much distended; the *pylorus* was shut, and the bread and milk, which the dog had taken with the *opium* about ten hours before, was indigested. There was nothing like chyle in the *duodenum*, nor any lacteal vessels to be seen in the mesentery. The bladder of urine and great guts were much filled, nor had the animal evacuated either urine or *fæces* from the time he swallowed the *opium*. *Impetum faciens Hippocrati dictum*, p. 402. & 403. The learned Dr. *Haller* has also observed, that *opium* puts a stop to the peristaltic motion of the guts in frogs and other animals, *Act. Gotting.* vol. ii. p. 154.

† This my worthy Colleague Dr. *Aiston* observed with a microscope in frogs into whose stomach he had conveyed a few drops of a solution of *opium* in water. *Vid. Medical Essays*, vol. v. part 1. art. xii. And indeed the great distension of the heart and its auricle in frogs killed with *opium* (N^o 5. compared with N^o 3. 6. & 10. above) indicates a more than ordinary resistance to the blood's motion in the arteries, as well as a less degree of irritability in the heart. Further, is not the slow, full pulse, and dry parched mouth in those who have got an overdose

lus of the blood with which it is distended, contracts more feebly and at greater intervals, till at last it ceases from motion altogether?

ART.

dose of *opium*, owing, partly to the slower motion of the fluids in the small arteries and secretory vessels of the glands? Tho' it must be confessed, that the dryness of the mouth may be in some measure owing to the perspiration being greatly increased by the *opium*.

ART. XXI.

*The History of a compleat Luxation of the Thigh, in a Letter to Dr. JOHN RUTHERFOORD, President of the Royal College of Physicians, and Professor of Medicine in the University of Edinburgh ; by JAMES MACKENZIE, M. D. late Physician at Worcester *.*

SIR,

Feb. 10. 1755.

“ THE account which I gave you of a
 “ compleat luxation of the thigh re-
 “ duced at the *Worcester* Infirmary, I now
 “ send in writing, agreeably to your request.
 “ I send you also Doctor *Wall*’s Letter vouch-
 “ ing this dislocation ; and Mr. *Jefferys*’s
 “ narrative of the same case. If you think
 “ any further proof necessary, there are still
 “ two Physicians and two Surgeons more,
 “ alive and well, who were present at the
 “ reduction, and will bear witness (if requi-
 “ red) to the truth of this accident, as well
 “ as the two Gentlemen above named, and
 “ myself, who am, &c.”

WILLIAM

* March 5. 1755.

WILLIAM JONES, a tall, robust, healthy Butcher, fifty six years old, was carried to the *Worcester* Infirmary on the seventeenth of *August* 1747, lame, and in grievous pain. His account of himself was, That he happened unfortunately, some hours before, to ride an unruly horse, which ran away with him; and that, making an effort to check him, just as he was galloping over a stone bridge, the animal reared himself upon his hinder feet; and immediately fell backward on his rider. The man was stunned with the fall; but the horse quickly recovered himself, and went off full speed, dragging poor *Jones* after him, by his leg which was engaged in the stirrup. The horse was soon providentially stopped, and the man set at liberty, but unable to move his thigh, which he believed was broken.

THE Surgeon in waiting, (for four Physicians and three Surgeons gave their attendance in rotation, namely, Doctors *Attwood*, *Mackenzie*, *Cameron*, and *Wall*; and Messrs. *Edwards*, *Russel*, and *Jefferys*) having thoroughly examined the situation and figure of the

the parts disabled and in pain, came to one of the Physicians, and told him, that the poor man's thigh was dislocated; that the head of the bone was struck quite out of the *acetabulum*, and lay fairly in the groin. The Physician having, in his younger days, attended *Boerhaave* (who, surely, understood physic and surgery as well as any man ever did), and knowing that, from the prodigious strength of the ligaments, and depth of the socket in that articulation, this learned Professor was of opinion, that the thigh-bone was never dislocated by external violence, but frequently broken near the head: which was the true reason why such accidents were seldom, or never, cured: the Physician, I say, persuaded, that his preceptor was in the right, observed to the Surgeon, that there must be a mistake somewhere, and that there was no instance on record, which could be depended upon, of such a luxation as he described: to which the Surgeon replied, "Sir, if you will not believe me, you will believe your own eyes and fingers presently."

THE novelty of the case brought all the Physicians and Surgeons to the Infirmary. A Skeleton was fetched, and great care taken

ken, by a strict inquiry into circumstances, to satisfy all present, that the bone was not broken, but really and fairly dislocated. There is no reasoning against facts: the toes and knee were turned outwards, the disabled limb was longer than the sound, the hip-joint utterly inflexible, and the round large head of the bone lay obvious to the sight and touch in the groin.

THE next inquiry was, how this dislocation should be reduced. All were called to consultation; not one of the Physicians or Surgeons had ever seen the case before. Some of the principal books of modern surgery were looked into; but one and all described the reduction, and recommended extension in such a general, languid, hearsay, manner, that it was plain they were as unpractised in the case, as the Gentlemen present: nor was any better success to be expected from *Galen's* * method of reducing
a

* Vid. Gal. in librum Hipp. de artic. commentarior. lib. 4. aph. 42.

And indeed the antients seem to have been acquainted with luxations of the hip-joint only in children, or distempered bodies; unless we shall except *Paulus Ægineta*,
whose

a luxation of this joint by hanging the patient to a strong beam by the heels with his head near the ground.

AFTER mature deliberation, it was agreed, that, in case the usual extension did not succeed, the *vis percussiois* (which is well known to increase the force to a surprising degree by accelerating the motion) should next be tried. In order to both, therefore, we provided a large strong table, of a proper length and height, which we fastened with screws to the floor, and covered with such blankets and bolsters as we wanted; a piece of strong cloth also was laid upon the blankets, under the patient's back, of length sufficient to turn up between his thighs, and pass over his shoulders down to the floor, where both ends were securely fixed, with a view to resist or counteract the necessary extension. We provided also two towels of a convenient length and thickness; one of which, at the middle, was tied with a tight, but easy knot above the patient's ankle, and the two ends twisted together, were given to three

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strong

whose various methods of extension, in this case, whether real or supposed, some of the moderns have copied, and some have altered.

strong men to hold. The other towel was in the same manner fastened above the knee, and the double end given to three more ; while the Surgeons stood ready, one with his hand on the ball of the dislocated bone to direct it into the socket, one at the knee, and another at the foot to turn them inwards.

When all things were ready, the extension was begun, in the common method, by the towel-men ; but tho' they exerted their utmost strength, the head of the bone was not moved in the least, and their effort served only to increase the poor man's torture to an intolerable degree.

FINDING thus the extension of no significance ; and the patient's courage reviving after some respite ; the *vis percussio*nis was carried into execution after the following manner. The towel-men were directed to slacken their towels to a certain point, to stand with their feet firm, their arms streight, and their bodies bending a little forward ; and, upon a certain signal agreed on, were ordered to pull with a vehement and quick jerk, throwing themselves back with all their might.

AFTER

AFTER every thing was in good order, and the assistants apprised of the nature and necessity of the operation which they were about to perform ; the signal agreed on was at last given. The towel-men pulled in a moment with a strong and sudden spring ; the Surgeons performed their parts dexterously ; and instantly there was a loud crash heard, which made one of the Physicians call out, Alas ! the table is broken. But at that very moment the patient, with a thundering voice, cried, It's in, it's in, it's in. And so it really was ; for we immediately found the limb restored to its natural position, length, and flexibility. The patient was put to bed ; and, by a proper diet and care, recovered his former health, and could walk perfectly well in three weeks. One of the Physicians often met him afterwards on foot driving cattle, and always asked, How he did ? To which his constant answer was, Very well, thank God and the Gentlemen. And he can now, upon occasion, walk twenty miles in a day, without fatigue or pain, tho' the injured limb still remains near a quarter of an inch longer than the other.

ATR. XXII.

*Some Observations on the new Method of curing the Cataract, by extracting the Crystalline Humour ; by THOMAS YOUNG Surgeon in Edinburgh *.*

TO restore lost sight, is recovering one of the most useful of all the senses, and the couching of the cataract, would be one of the most valuable operations, could it always be done with safety ; but the bad success, and dreadful consequences which often attend it, have deterred many good Surgeons from performing this operation, and thrown it much into the hands of empirics.

I have couched but few in the old way, and those with such bad success, that I was fully determined to operate no more on the eyes ; nor did the success of the new method performed by the ingenious M. *Daviel*, alter my resolution for a considerable time, till, at the importunity of some of my best friends,

* March 4. 1756.

I consented to try this new operation. Six cataracts luckily cast up last summer in the Royal Infirmary at *Edinburgh*, which I extracted in the following manner.

THE patient being seated in a chair, with an assistant at his back, to support his head, and keep up his eye-lid, as in the old operation, the operator may stand or sit in a chair, as he finds most convenient.

HE should keep down the under eye-lid with two fingers of the one hand, while with the other, he takes the small knife A (*Plate VII. Fig. 3.*) with which he pierces the transparent *cornea* at the external angle of the eye, near to where the *cornea* joins with the *sclerotica*, taking great care not to wound the *iris*. Run the knife in a horizontal direction across the anterior chamber, and bring it out about the same distance from the white of the eye, as where it entered; then cut that part of the *cornea* which lies below the two orifices, as much in the form of a crescent as possible, this makes the incision larger, and keeps the cicatrice more off the sight; lift up the flap of the cut *cornea* with the scoop B (*Fig. 4.*) or any other convenient instrument; introduce at the
same

same time a common couching needle C, (*Fig. 5.*) thro' the pupil, to open the *capsula* of the crystalline *lens*, that the latter may come the more easily out. A small aperture generally serves this purpose; if the *lens* is of a firm consistence, it often sticks to the point of the needle, so that when the instrument is withdrawn the crystalline comes along with it; if it does not, a very gentle pressure upon the eye forces it out. The operation may be frequently performed with the knife alone, the *capsula* of the crystalline being sometimes so thin, that, after the *cornea* is cut, a small pressure on the eye makes the *lens* come away.

THIS method of operating is much the same with that practised by M. *Daviel*, which you'll find at large in the *Memoirs of the Academy of Surgery*, *vol. ii. p. 337.* I have followed the example of the famous Mr. *Sharp*, and shunned the great multiplicity of instruments M. *Daviel* makes use of, which renders this operation more simple, less tedious, and less dangerous.

I shall next mention the success of each operation in the order they were performed.

I. ROBERT

I. ROBERT LAURIE, aged about 30 years, was admitted into the Royal Infirmary with a cataract in both eyes.

I operated on the left eye the 23d of July 1755.

As soon as I had passed the knife into the anterior chamber, he turned his eyes so much upwards, that the *cornea* was quite out of sight ; I waited till the eye returned to its former position, when I found the point of the instrument in the *iris*, which I immediately disengaged, and finished the operation without any other accident.

I expected a great inflammation from the *iris* being touched, but was agreeably disappointed, finding the man recover with little pain, no fever, and the inflammation inconsiderable.

ABOUT three weeks after the operation, he could distinguish colours, and large objects tolerably well ; but could not bear much light. His eye continued weak and watery for about three weeks more, when he could easily see a pin in the sleeve of his own coat ; his eye was clear, but the pupil not quite round, which was certainly owing to the *iris* being hurt.

2. ————— was admitted into the Royal Infirmary about the middle of *September*, with a cataract in the one eye, and the *cornea* of the other quite opaque.

THE pupil of the cataracted eye was contracted to above the size of a large pin head, but quite immoveable.

HE was visited by several Surgeons in town, who were of opinion, that the disease was incurable, and that the bottom of the eye was affected, as well as the crystal-line *lens*.

I proposed trying the new operation, before he should be dismissed incurable; to which they very readily consented.

I performed it without any accident, and the man recovered in a few days, without any fever, pain, or inflammation. He was dismissed the house about a fortnight after the operation, when his eye was quite clear, but the pupil still immoveable; and he could only perceive a glimmering of light, which is more than was expected from the appearance of the pupil before the operation.

3. & 4. JOHN CRAIG, aged about 40 years, was admitted into the Royal Infirmary with

with a cataract in both eyes, which had much of the milky appearance.

I operated on both eyes the 28th of *September* 1755, and nothing extraordinary occurred during the operation ; only, upon dividing the *capsula* of the cryftalline, a fort of milky liquor came out, and the *lens* was of a dark-brown colour. He had a very speedy recovery ; fix days after the operation, I uncovered his eye ; he was capable of distinguishing colours. I looked again into his eyes on the 13th day, when I found his fight ftill better, and his eyes more able to look at fmall objects, without complaining.

HE was difmiffed the houfe the 10th of *November*, when he could read without the affiftance of glaffes.

5. ROBERT LAURIE, whom I have already mentioned, had the operation performed on the right eye the 12th of *October*, when nothing extraordinary happened ; he had a very good recovery, with fcarce any pain or inflammation ; he was difmiffed from the houfe the 19th of *November*, when he faw very diftinctly with both eyes.

16. AGNES BARROWMAN, aged about 30 years, was admitted into the Royal Infirmary, with cataracts in both eyes.

I operated on the left eye the 26th of October 1755.

THE space betwixt her eye-lids, when raised up, was so small, that I could with difficulty see all the *cornea*, which, in this patient, was remarkably flat.

As soon as I had passed the knife into the anterior chamber, she was seized with a fit of coughing, which obliged me to cut the *cornea* in a very great hurry. The opening in the *cornea* was but small, which gave me more difficulty in extracting the crystalline, than I had in any of the former.

NOTWITHSTANDING this unlucky accident, she had a tolerably good recovery; her eye was pained, and somewhat inflamed, for sometime after the operation, but never violently. She was dismissed the house about six weeks after the operation, being then able to distinguish very small objects.

N. B. SOME eyes are more proper for this operation than others; the larger the eye, and the more convex the *cornea*, the operation will be the easier. This woman had a remarkably

remarkably bad eye in this respect ; it was small, the *cornea* flat, and the distance between the eye-lids, when open, was very little : perhaps the *speculum oculi* would be of use to help all these faults while the *cornea* is cutting, but no longer, for fear of pressing out the vitreous humour.

THERE was nothing particular in the treatment of these patients after the operation ; it consisted chiefly in bleeding, spare diet, now and then a gentle laxative, and cloths dipt in vinegar and water applied frequently to the eyes ; they were not confined to their beds above a day or two, and none of them required fomentations.

I do not pretend, from the above cases, to make a comparison betwixt the success of couching, and the new method ; this requires more cases than I have had occasion to see.

ACCORDING to the trials made by some of the *French Surgeons*, which you'll find in the *Memoirs of the Academy of Surgery*, vol. ii. p. 578. the couching was the most successful.

MR. *Morand* couched six patients.

3 of them saw distinctly.

3 of the cataracts rose again.

M.

M. *la Faye* extracted six cataracts in the new way.

2 of the patients saw distinctly.

2 of them saw less distinctly.

2 of them were quite blind.

M. *Poyet* extracted seven cataracts after the new method.

2 of his patients saw distinctly.

2 of them less distinctly.

1 could distinguish light.

2 of them were quite blind.

WERE I to judge from my own experience in both operations, the new method certainly claims the preference ; since I have only operated upon six cataracts, and all of them have succeeded, tho' some were not very promising.

This, I hope, will excite others to make further trials and improvements in this operation.

ART.

ART. XXIII.

*A Hernia from the Omentum falling down into the Scrotum ; by THOMAS LIVINGSTON, M. D. Physician at Aberdeen *.*

A Young man, aged about 28, of a very thin habit of body, and naturally of a healthy constitution, was seized with a pain and uneasiness about the region of the stomach ; for which complaints, (without any regular advice) he took a vomit, which operated in the usual manner, but without alleviating his former uneasiness. In the evening of that day, about six hours after the operation of the vomit, he was seized with sharp pains over all his belly, for which, by the advice of a Physician, he was ordered an emollient clyster, and an anodyne at bedtime. The clyster operated gently, and he passed the night pretty free of pain, till towards morning that the pain returned more violent than ever ; particularly in the left side of the umbilical region, and in the left testicle.

* February 5. 1755.

testicle. I was called to visit him that forenoon, and upon examination found a scrotal *hernia* on the left side, about the size of a large fist, extremely hard over all its surface, and very painful on the slightest touch. He had a hard frequent pulse, intense thirst, and all the other symptoms of inflammation. He was immediately bled to a considerable quantity, warm stupes wrung out of an emollient decoction were alternately applied upon his *scrotum* and belly, and an emollient purgative clyster was injected. The application of the warm stupes gave him some small relief, and he imagined the parts were softer ; but, upon using the *taxis*, the pain was intolerable, and it was in vain to endeavour to reduce the *hernia*. He continued in this state till the evening, when his fever demanded a second bleeding, which was accordingly performed ; another stimulating clyster was thrown up, and soon after he had two or three copious dejections of indurated *fæces*. The operation of the clyster gave him some ease ; but the state of the *hernia* was not in the least altered. He continued restless and much pained all night, and in the morning he drank the *decoct. tamarindor.*

marindor. cum dupl. sennæ, which gave him several loose stools throughout the day, the *hernia* continuing as hard and painful as formerly.

HE would not agree to have the operation of the *bubonocèle* performed, and in the evening his pulse became feeble ; he had frequent returns of a *singultus*, and died next morning. It may be necessary to mention, that soon after the vomit he cried out, that something was tearing his stomach and guts towards the bottom of his belly.

DURING his illness I was much perplexed about the nature of the *hernia*. As the clysters and ptisan had operated very naturally, I could not imagine that it was any portion of the intestines ; and as he was of a remarkably thin habit of body, I could scarcely imagine that it was the *omentum* ; however, as his friends gave me the liberty of examining the body, my doubts and scruples were soon satisfied. I first looked the state of the *abdomen*, where the following appearances were very obvious. The *omentum* fallen down, greatly stretched, and so tense, that one should have imagined it would have broke. The stomach much distended

tended with air, the great curvature of it much lower down than its natural situation; the great arch of the *colon* quite out of its place, and lying as low as the middle of the small guts; the *jejunum* and *ileum* considerably inflamed and much distended with air, and the mesenteric vessels much more turgid than usual. These were the principal things to be observed in the *abdomen*. Upon making an incision through the teguments of the *scrotum*, (in the same direction as is ordered in the operation for the *bubonocoele*) I soon discovered the hernial sac, which was very thin, tense, and rigid; and, upon laying the sac open, there was nothing to be found but the *omentum*, which was compleatly mortified as high as the ring of the muscle. Upon dilating the ring itself, I found a convolution of the *ileum* sticking in the very mouth of the opening, but the one half of the transverse diameter of the canal was only engaged, and that part of it which was strangulated was in a mortified state. The remaining part of the gut betwixt that and the *caput coli* was much smaller than usual, considerably inflamed, and contained a little
 putrid

putrid viscid chyle. Upon taking out the *omentum*, I found a resistance towards the lower part of the *scrotum*, which I imagined was owing to some adhesion; but, upon using a very gentle force, it was easily extracted, and plainly came out of the *tunica vaginalis testis*, in which there was a pretty large hole or perforation. The testicle appeared to be quite sound. That portion of the *omentum* which was contained in the *scrotum*, weighed six ounces and a half.

ART. XXIV.

*A Child brought forth at a Rent of the Belly **

IN *April 1736, Elspet Grant*, in the parish of *Moy*, being with child, took her labour-pains. After they had continued three days with the child in the birth, two cracks, as if the rafters of the house had broke, were heard about the sick wife, and her belly was rent from near the navel, with a squaint downwards and to the left side, to near the share-bone. At this rent the child came into the world, the after-burthen was brought away, and the intrails were seen.

THE rent was cured without any other application, than that of butter mixed with white sugar, and its scar was only as the scratch of a big pin.

THESE facts are attested by the judicial oaths of *Anna Kennedy* a midwife, and *Mary Ogilvie* a neighbour, who were present when the rent was made and the child came out of it; of *Margaret Dallas*, who assisted to bring

* May 1. 1755.

bring away the after-burthen ; of *Robert Smith* who saw the rent and intrails immediately after this ; and of *Isobel Tarrel*, who afterwards examined the scar : taken and subscribed by *James Macqueen* younger of *Corribrough*, Bailie to the Laird of *Mackintosh*, at *Moyhall*, November 22. 1738 ; of which the original subscribed copies are kept by the Secretaries of the Philosophical Society of *Edinburgh*.

A Child escaping at a Rent of the Womb into the Abdomen ; by ALEXANDER MONRO, Senior, M. D. & P. A.

IN March 1744, I was desired by Mr. *Ramsay* Surgeon here, to witness the examination of the body of a woman who died in child-labour without being delivered. The account given me of this woman was, that she was about 35 years of age, and had born two dead children, and a living one. Being at her full reckoning, her pains had begun on *Tuesday* morning, and continued in a natural way, the child advancing towards the birth,
and

and some of the waters coming away all that day and *Wednesday*, till *Wednesday* evening, when sitting in a chair in labour, she gave a sudden spring from the chair, complaining of violent pain in her belly. The child never was felt afterwards by those who attempted to assist her delivery. She constantly complained of violent pain in her belly, with her senses and judgment intire, till *Friday* morning, when she died. On *Saturday* her body was opened.

AFTER cleaning away with sponges a considerable quantity of blood floating in the *abdomen*, we saw a ripe child and its secundines lying in the lower part of it, a little to the right side. The child, *placenta*, and umbilical rope were intire, and the membranes were as usual after birth. The woman's *uterus* had its *fundus* raised as high as the navel, with its substance soft and spongy as is common in pregnant, nothing preternatural appearing in its fore-side; but when the *fundus* was turned down and forwards towards the *ossa pubis*, a large rent four inches long was seen towards the neck of the womb; which being again put into its natural situation, was opened its whole length, in the middle

middle of its forepart, when we had a better view of the rent, extending from very near the *os uteri* upwards, a little obliquely to the right side. The *os uteri* was then very little open. The *cervix* which is distinguishable plainly from the *fundus* in a woman not with child, was here extended into the same common sac with it. The inner surface of the womb was all smooth, seeming to be covered with a fine villous membrane. From the larger size of the sinuses at the back and upper part of the womb, I judged the *placenta* to have been formerly applied there.

ART.

ART. XXV.

*A preternatural Collection of Waters in the Womb with Twins ; by STEPHEN FELL Surgeon in Ulverstone *.*

IN November 1747, I was called to *Hannah Saltbouse*, mother of several children, then, according to her reckoning, in the first week of the eighth month of her pregnancy. She had observed her belly increasing in bulk very fast during the preceeding month, and particularly in the two last weeks, in every day of which a sensible difference was said to have been observed. At my first visit her *abdomen* appeared to me much more distended than ever I had seen in a woman with child, especially from the navel to the upper part of the epigastric region, where it was more tense than towards the *ossa pubis*, and the most stretched part had little sense of feeling. Her pulse was high and quick, her breathing was difficult, her
face

* February 6. 1755.

face was cadaverous, and she had continued unnatural labour-pains, with much thirst and little urine. The *os tincæ* was a little opened, but without any forming of water that could be felt. I ordered clysters and an opiate at night, which made her pass it quietly.

NEXT day the pains were more natural, and she was tolerably easy. In the evening, she took an opiate with two scruples of *pulv. ad partum*, after which, there were intervals between her pains; but at four in the morning, she became delirious, and appeared like one who had not an hour to live. On taking rich hot negus, these symptoms went off; and then having felt a child's head floating in water, I broke the membranes, and brought away two male children, who were scarce half the size of children born at the usual time. One of them was dead, swell'd and livid; the other lived 64 hours, but without taking food, or making any discharge by stool or urine. The quantity of water voided in this delivery was computed by all present not less than six wine gallons, 48 or 50 *lib*.

My

My patient's belly subsiding to the natural dimensions, was swathed immediately after the after-burthen was brought away. She had strengthening cordials and anodynes given her, with a proper diet, and sometimes clysters. Now, on the 12th day after her delivery, she appears out of danger.

ART.

ART. XXVI.

*Histories of tophaceous Concretions in the alimentary Canal; by ALEXANDER MONRO Senior, M. D. F. R. S. and Professor of Anatomy in the University of Edinburgh *.*

THE number of histories of tophaceous concretions in the alimentary canal is not so great, but that your collection might, in my opinion, admit of some few, especially if there is a variety in them.

Hist. 1. A healthy boy, about twelve years of age, began to complain of colic pains, which increasing with frequent gripes, *borborygmi* and vomiting, had such an effect, that his parents asserted, he was scarce of so large stature after six years of his disease as he was at the beginning of it. Vomits, purges, vermifuges, attenuants, and a variety of other medicines had been given in that time without any benefit.

His father, one of the town-officers or serjeants, having then asked my worthy friend

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and

* February 6. 1755.

and colleague Dr. *Plummer's* advice, he desired my assistance. We were told by the lad, who was greatly emaciated and very weak, that some years past he had not had the vomiting, but found a hard painful *tumor* above the left groin, which sometimes shifted place a little, where he fancied often he felt something like the striking of two hard bodies on each other. He had been of late much fatigued with *tenesmus*. Sometimes he had no excretion of *fæces* for several days, and often he could scarce make any water, and that only in drops. During two days before our visit, the *tenesmus* was constant, and he felt something hard within the *rectum* near to the *anus*, which he and several others had endeavoured in vain to bring away with their fingers.

ON extracting this substance with a *forceps*, such as is used for extracting stones from the bladder, he was much easier than he had been of a considerable time. Next day, he passed two other balls, and on each of the two following days, a ball, which he could not force out at the *anus*, was extracted with the *forceps*. After this, he had no uneasiness,

ness, and soon became a healthy strong young man.

THE largest of these five balls which was the first extracted, is five inches in circumference and something globular, but with several prominences and flat surfaces. Most of the flat parts had a smooth shining tartarous thin coat, the rest of it was more rough and of a spongy appearance. The two last brought away, are less in bulk, and without so much tartarous crust. The two small ones are all covered over with the shining tartarous crust, which in several places is prettily variegated with different shades of an ashy colour. One of them has some resemblance in its shape to the shell of a tortoise. The other or smallest may be compared to two pyramids joined by a common base.

THE second in size and the smallest are cut thro' near to the middle, where there is a small flat bone, that probably has been the *nucleus* about which these balls were formed, tho' they are not of the same shape.

WE were informed by the parents, that they had often chid their son for swallowing the small bones of sheep and lambs feet, the
finewy

finewy parts of which, when boil'd, the family frequently took for food.

Hist. 2. A man who had been long tortured with a painful hard swelling in his belly a little above the right groin, which frequently caused vomiting and *diarrhœa*, tho' at other times he was very costive, asked my advice, when he was very weak and emaciated. The seat of this *tumor*, the kind of feel it had thro' the containing parts, and its tumbling, as the patient said, from one place to another, when he changed postures, made me suspect a concretion to be lodged in the great sac of the *colon*.

In hope to push it forwards in the *colon*, I caused his great guts to be filled with whey injected by the *anus*, and then directed himself and assistants to press repeatedly the *tumor* upwards. This *manœuvre* being several times renewed without success, I prescribed a brisk purgative, and ordered the injections with the pressure to be repeated as soon as the cathartic began to operate. But this and several other such operations failing, my patient died.

HAVING obtained leave to examine his body, my conjecture proved to be right ; for
in

in the *caput coli* there was a ball of more than seven inches circumference, with a depression at opposite ends. The intestine had contracted so much at the side of the ball next to the cavity of the *colon*, that I could not force it thro' the aperture there, but was obliged to cut the sac in which it was lodged to take it out.

THE ball had no tartarous crust on its surface, and when it was cut thro', its *nucleus* was a chalky or limy substance about the size of a common pea.

Hist. 3. DR. *John Stevenson* Physician gave me a concretion six inches in circumference, the *nucleus* of which is a plumstone, taken out of the intestines of a boy of five years old. Tho' the stone had been swallowed long before the boy's death, the kernel of it was fresh when the stone was taken from the middle of the ball. A clyster had brought away several other plumstones from this boy some months after the plums had been eat.

Hist. 4. DR. *Stevenson* also gave me another such concretion, which has four flat sides with several depressions in them, measuring about five inches in circumference, formed

formed also on a burnet plumstone, which he took out of the intestine of a girl.

Hist. 5. FROM the same Gentleman I likewise had a third concretion pretty like to, but a little larger than, the one described and painted by Dr. *Simson* in *Med. Ess.* vol. i. art. 32. which, with three such others, he took out of the intestines of another patient. Each of them had a small stone in the middle, the patient having formerly swallowed small stones and pebbles, for what he called a colic in his stomach.

THE Dr. tells me, that all these three patients wasted, without being sick or losing their appetite. They were fond of flesh for food, and were averse to fops. They seldom were free of *borborygmi*, which made the *abdomen* to change almost constantly its appearance, the parts of it rising and sinking as the air went from one place to another.

Hist. 6. In a ball of this kind eight inches one way and six the other, taken from a Gentleman's intestines, whose history I do not know, the *nucleus* is a little round piece of wood about the size of a common hazle nut.

NONE of the balls mentioned in these four last histories have any tartarous crust, but they and all the other cut ones have the appearance of being composed of *strata* surrounding the *nucleus*, their colour differing in shades from a dark rusty to a pale ashy colour. Their substance, except where there is tartarous crust, resembles a fine hat or chamois-leather when cut.

Hist. 7. IN the collection of curiosities kept by the Surgeons of this place, there is a ball taken out of the stomach of a horse, which is nearly spherical, and nineteen inches in circumference. Its surface has something of the mulberry form, being composed of a great number of hemispherical knobs, about a quarter of an inch diameter contiguous to each other. Their outward shell looks like a thin crust of sandy clay; but within this the substance has the same matted appearance as the human concretions have.

Hist. 8. BALLS are also frequently formed in the stomachs of cows. Three of them which were given to me, are almost exact spheres of a black colour, composed

posed of an external hard tartarous crust, which is about $\frac{1}{20}$ of an inch thick. Within this there is nothing but short black hairs matted compactly together. The circumference of the largest is 9 inches ; of the second, 6 ; and that of the third, is $5\frac{1}{2}$.

ART.

ART. XXVII.

Remarks on Procidentia Ani, Intususceptio, Inflammation, and Volvulus of the Intestines; by ALEXANDER MONRO Senior, M. D. & P. A.*

AT stool more or less of the inside of the *rectum* is generally thrust out beyond the verge of the *anus*, which ascends when the pressure of the diaphragm and abdominal muscles ceases. If the protruded intestine is not then retracted, it is squeezed by the *sphincter ani*, so that the return of liquors from the part of it which is beyond this stricture, must be rendered difficult; on which account, this part swells, becomes of a colour more red than natural, and a larger than ordinary quantity of slime flows from the ends of the vessels that open on its inverted villous surface. This state is called *procidentia ani*; a disease, to which children, old people, those weakened by diseases, or such as are attacked by *tenesmus*, from

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whatever

* February 6. 1755.

whatever irritating cause, are more subject than others.

THE speedy reduction of the inverted protruded part of the intestine, is the effectual cure ; nor should time be lost, as is often advised, in trying to diminish the swelling by warm fomentations and poultices, which relax the vessels, and rarify the liquors, and therefore produce an effect very different from what they are intended to have. If the prolapsed intestine is so much swelled, that it cannot be made to pass thro' the contracting *sphincter*, incisions may be made in its surface, by which part of the slime and blood contained in the cellular membranes, may be squeezed out, to diminish its volume, and thus to make it capable of returning again within the body.

THE practice of the nurses and other good women, in making the reduction of a *pro-cidentia*, is very faulty. They apply a warm cloth to the protruded part of the gut, and pressing on it, endeavour to thrust it all up at once. Before the intestine swells, this operation sometimes succeeds with children whose *sphincter* is weak. But dry cloths or fingers are liable to adhere to the villous coat
and

and give great pain in taking them off. The cloths or fingers applied here ought always to be besmeared with oil, unsalted butter or axunge, to prevent this adhesion. The bulk of the prolapsed part is often so great, that it is impossible to make it all pass at once thro' the *Sphincter*, and a fruitless attempt of reduction generally increases the swelling. The reduction ought to be made by pressing a small part of the sides of the orifice, with a greasy finger, and when that part is thrust within the orifice, another finger is applied to what is then the verge of the orifice to push it upwards, while the first applied finger is withdrawn: by such an alternate succession of two fingers, the whole may be introduced in most cases without incisions, so that this disease is seldom fatal; and for that reason, the patient is generally too much neglected after the reduction is made, which is sometimes attended with bad consequences.

IF, after the reduction, the part continues to be pained and the patient's pulse is quick, blood-letting, and a low cooling diet, are necessary to prevent inflammation and its consequences. In all cases, too much costiveness
and

and its contrary a *diarrhœa*, especially with *tenesmus*, are equally to be guarded against ; seeing a return of the *proidentia* may be caused by either of them. The relaxed parts are to be braced by strengthening topical medicines. In the astringents commonly prescribed, I can have no confidence ; their effect goes no deeper than the skin ; but stimulants, such as ardent spirits or tinctures of the aromatic resins made with them, give a spring to all the parts, and excite a glowing heat whenever they are applied, so as to touch any part of the extremity of the gut, which they can always be made to do.

I have said, that this disease, the *proidentia ani*, is seldom fatal ; and the reduction of the prolapsed part of the intestine into the body, is generally regarded as a cure of it ; but that this is not true, when the doubling of the intestine is high up, will appear from the following history.

A large-sized strong healthy boy, a year and an half old, after a *diarrhœa* of some days, with *tenesmus*, was observed to have a *proidentia ani*, which was treated two days by the women who attended him ; after which Mr. *Adam Drummond*, Surgeon in this place,

was

was called to his assistance. He reduced the *proidentia* frequently ; but it soon returned, which made him desire I should be consulted.

THE inverted intestine stood out four inches from the *anus*, without being much swelled or of a deep red colour, and the child seemed to have no other disease. Mr. *Drummond* most easily introduced all the *tumor* into the body ; but soon after it was pushed out again, upon the child's having a desire to stool, notwithstanding a servant's keeping a finger on each side of the *anus* near to each other, while some liquid excrement was passed. After the reduction was again made, I put my finger, which is long, up the *rectum*, pushing the orifice of the inverted gut on the point of it, and then found the orifice of the inverted gut resembling the feel of the *os tincæ* of an unimpregnated womb resting on it, which I could throw up some way further with a sudden jirk of the last joint of the finger, but without being able to invert it. We then caused a large quantity of milk and water to be injected with force, while the two sides of the *anus* were pressed firmly on the pipe of the syringe introduced by the

anus

anus into the *rectum*, to prevent the liquor's recoiling, in hope that the liquor would carry the inverted gut before it, to cause its return to the natural situation. This operation being repeated several times in vain, the *prociſſentia* always returning with the *tenesmus*, a very long probe of whale-bone was made, a sponge was fastened round the probe, this was wet in oil, the probe was introduced into the orifice, which, I ſaid, reſembled an *os tincæ*, the ſides of which reſted on the ſpunge ; and with this the inteſtine was pushed a great way up into the body in the direction of the *rectum*, but without ſucceſs. Several attempts of the ſame kind failing, we deſpaired of a cure ; and the child, ſometime after, being attacked with ſevere vomiting and perpetual *tenesmus*, died in few days.

Mr. *Drummond*, who opened the body, told me, that the inverſion began a little below the upper part of the ſigmoid flexure of the *colon* ; and that the meſocolon was torn away from the inverted part.

WHEN a doubled part of an inteſtine is extended into the cavity of this alimentary canal,

canal, without appearing externally at the *anus*, it is called *intususceptio*, which, I am persuaded, is a much more frequent disease than is generally thought. I have seen several whom I judged to have died by it, and shall now relate the cases of four people whose bodies were examined after death.

1. A middle aged woman, during sixteen months before her death, suffered greatly from colic-pains, distension of her belly, vomiting, and *tenesmus*. In the latter part of her life, when I first saw her, she had no cessation from pain, except by the force of *opium*.

IN the great arch of the *colon* was a doubled part of that gut, seven inches long. The containing intestine had a very slight adhesion to the inverted doubled part contiguous to it. The doubled part was of a dark red colour, but not very hard. The passage for the *faeces* thro' it was very narrow, not allowing a finger pushed with force to pass.

2. A woman about fifty years of age lived two years with such symptoms as were narrated in the preceeding case. We found a doubled part of the *colon* four inches long in
the

the left loin, with the same appearance as described in the former history.

3. A girl seven year old, after eating a carrot and some curren-berries, had a colic which continued with a distended belly, vomiting, and passing little *fæces*, from *July* till the middle of *December*, notwithstanding various medicines were given.

MR. *Malcolm* Surgeon in *Dalkeith*, whose patient she was, being allowed to open her corpse, cut out the affected part of the intestine, and sent it to me. The end of the *ilium*, *valvula Tulpii*, *caput coli*, and *appendix vermiformis*, were raised twelve inches within the *colon*, to which they had a slight adhesion. The outer surface of the contained intestines was dark-coloured, and very unequal. The orifice of the prolapsed part was not at its end, but at one side an inch and a half from the end, with a soft flexible prominence at each side of the aperture, which I judge to have been *Tulpius's* valve. The doubled parts were so grown together, that I could not distinguish one from the other. The passage within them was so small and crooked, that I could not push a probe thro'

thro' it ; but cutting it open gradually, I found it was still pervious.

4. DR. *Cullen*, Professor of Medicine in the University of *Glasgow*, communicated the following case to me.

A boy about twelve years of age complained of wandering colic-pains, which he imputed to blows received on his belly from some of his companions. These pains returned frequently with *diarrhæa*, and sometimes bloody stools, for near a year, when his parents consulted Mr. *James Muir*, Surgeon in *Glasgow* about him. The boy was then much emaciated, had a quick pulse, and was so weak as to be confined to his bed. Two weeks after this, a livid membranous substance, passed by the boy at stool, was brought to Mr. *Muir* ; who observing it to be tubular, tied one end of it, and blowing into the other, distended it into such a convoluted tube thirteen inches long, as you see represented A B C, *Fig. 1. of Tab. VII.* which I caused to be drawn from the original which was sent me. As it has the mesentery D connected to all its concave side, it appears to have been an intire piece of gut, and not the villous coat only. Besides this

large portion of intestine, there were several shreds and smaller pieces passed by the patient; notwithstanding which, Mr. *Muir* saw afterwards, among the boy's *fæces*, skins of potatoes which he had eat after these parts of the intestine came away, so that they had not made any discontinuity in the alimentary canal. The symptoms continuing, the boy died in six weeks.

Mr. *Muir* opened the body of his patient, in presence of several Gentlemen of the faculty, who saw what I am now to describe, with the assistance of a figure, which I caused to be taken of the dried preparation of the intestine sent me.

THE folds of the intestines and *omentum* were all glued together by a fatty curdy matter. Within four inches of the valve of the *colon*, the *ilium* A B C, *Fig. 2. Tab. VII.* formed into the usual curve by the mesentery D; suddenly rose perpendicularly at E, where it was much contracted and had the appearance of a cicatrice. When the intestine was opened, this contracted part of it was found much thicker and harder than it was any where else, especially on one side, where

where it stood so far into the cavity, as to leave a very small passage for the aliment. Along this contracted part, the mesentery F was firm and thick. After this, the intestine G became of a natural enough form and make.

THE Gentlemen in *Glasgow* were, I think, justly of opinion, that the part of the intestine inflated by Mr. *Muir*, delineated in *Fig. 1.* was an intususcepted part fallen away by gangrene from the intestine at E in *Fig. 2.* where, if there was a concretion, as is related in case 3. it might have separated without leaving any discontinuity in the alimentary canal.

I have several times seen an *intususceptio* in the small guts of children, a little below which I observed several worms ; but the inverted part was neither swelled nor discoloured, which made me think this disorder had happened soon before death. In one of them a *lumbricus teres* had passed the half of its length thro' a hole made in the gut ; but, as there was no redness or other mark of inflammation at this part, I judged the perforation to have been made by the worm after the death of the subject.

THERE

THERE is little difficulty in conceiving how a piece of a gut should enter doubled into the part below it, and how it may be gradually protruded downwards to a considerable extent by the food or *fæces*, in their descent towards the *anus* ; but it is not easy, when this disease begins, to distinguish it from a variety of other disorders which happen in the alimentary canal, or to find a remedy when it is suspected, especially if the *intususceptio* is in the small guts ; and from the observation above narrated of the child with the fatal *procidentia ani*, it would appear difficult to cure it ; nay, if the doubled parts of the intestine are grown together, as in most of the histories, a reduction of it is impossible. Nature seldom will perform what I imagine she did in the last case, separate the doubled part, and unite what contained it. Nor do I believe any will be so hardy as to advise the amputation of the affected part of the gut.

'Tis surprizing how the people in the preceding histories lived so long as they did, with such large doublings of the intestine, and its mesentery pressed together within another

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Fig. 1st

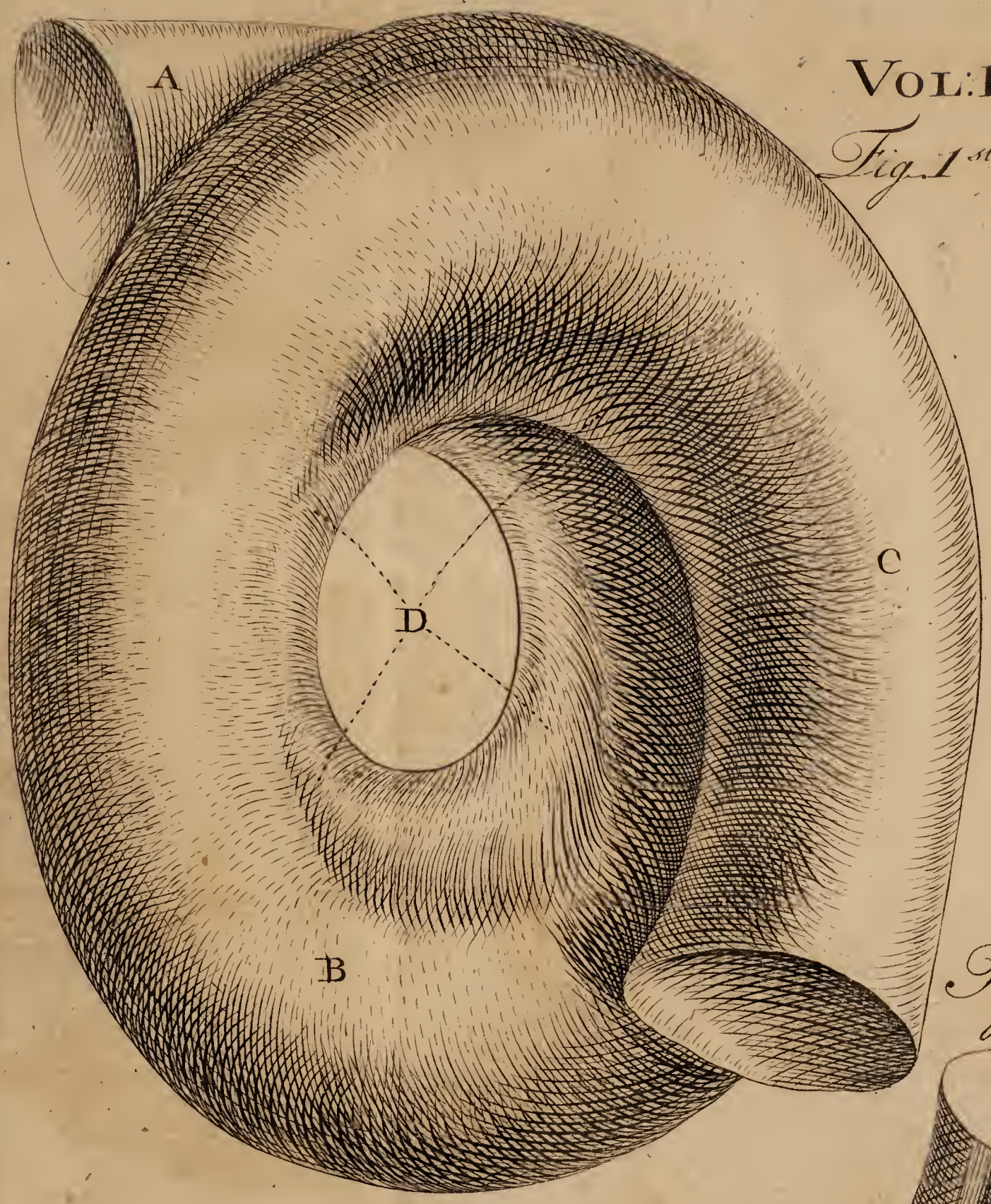


Fig. 2^d

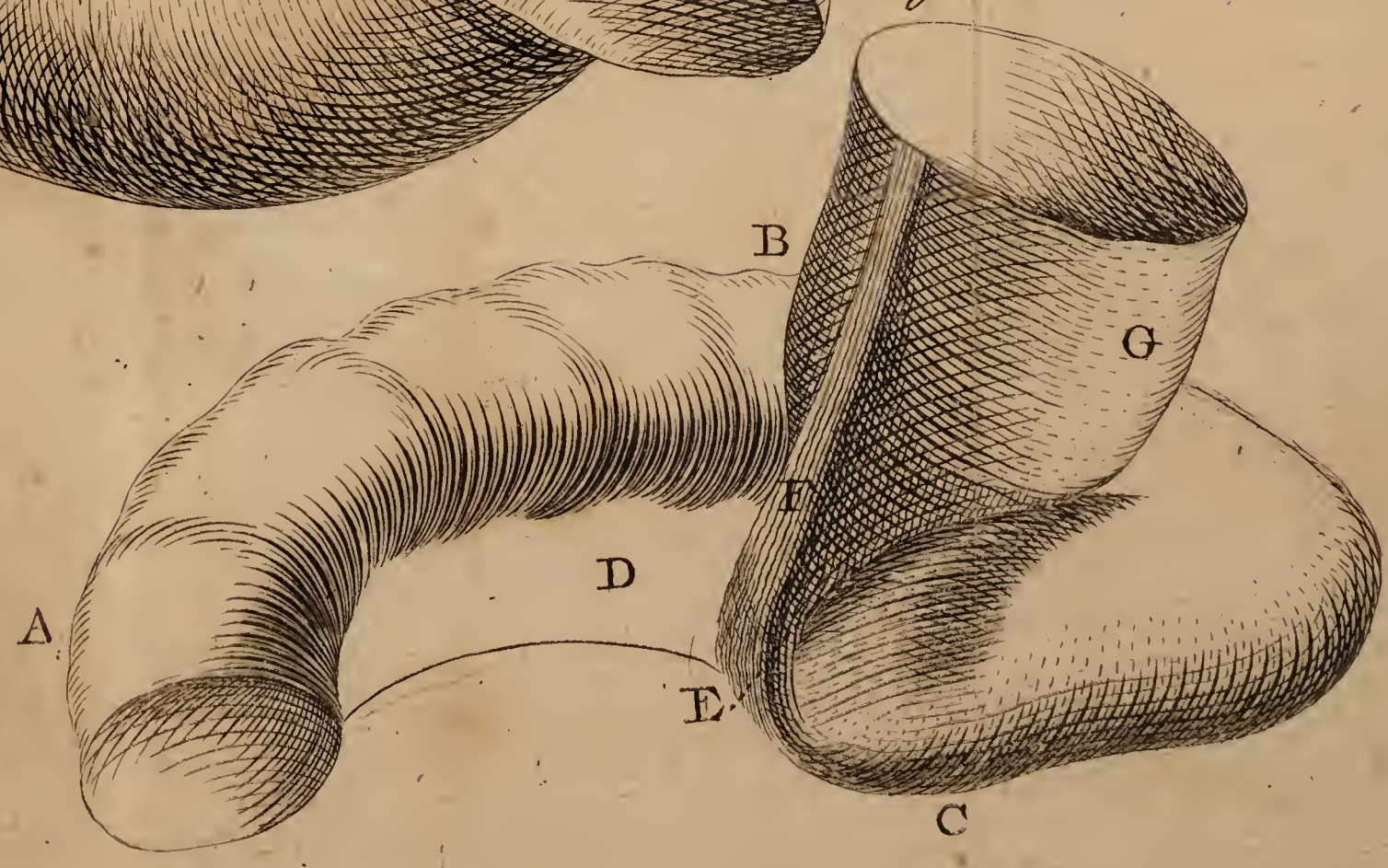


Fig. 3^d

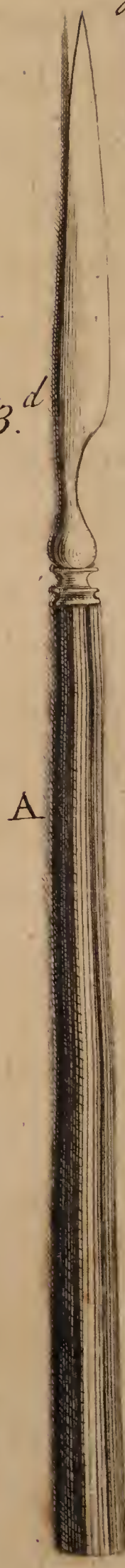
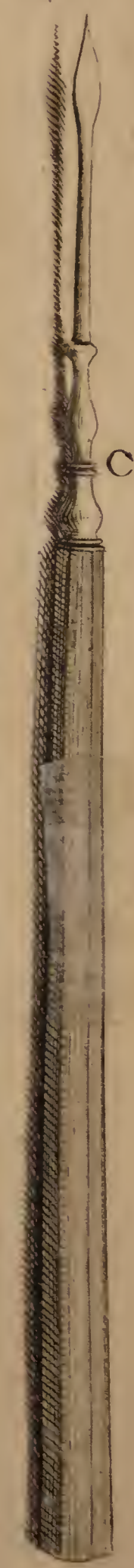
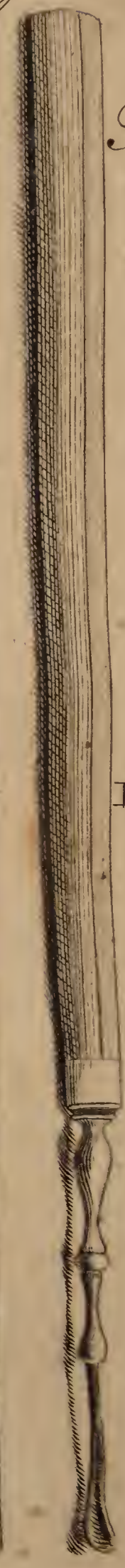
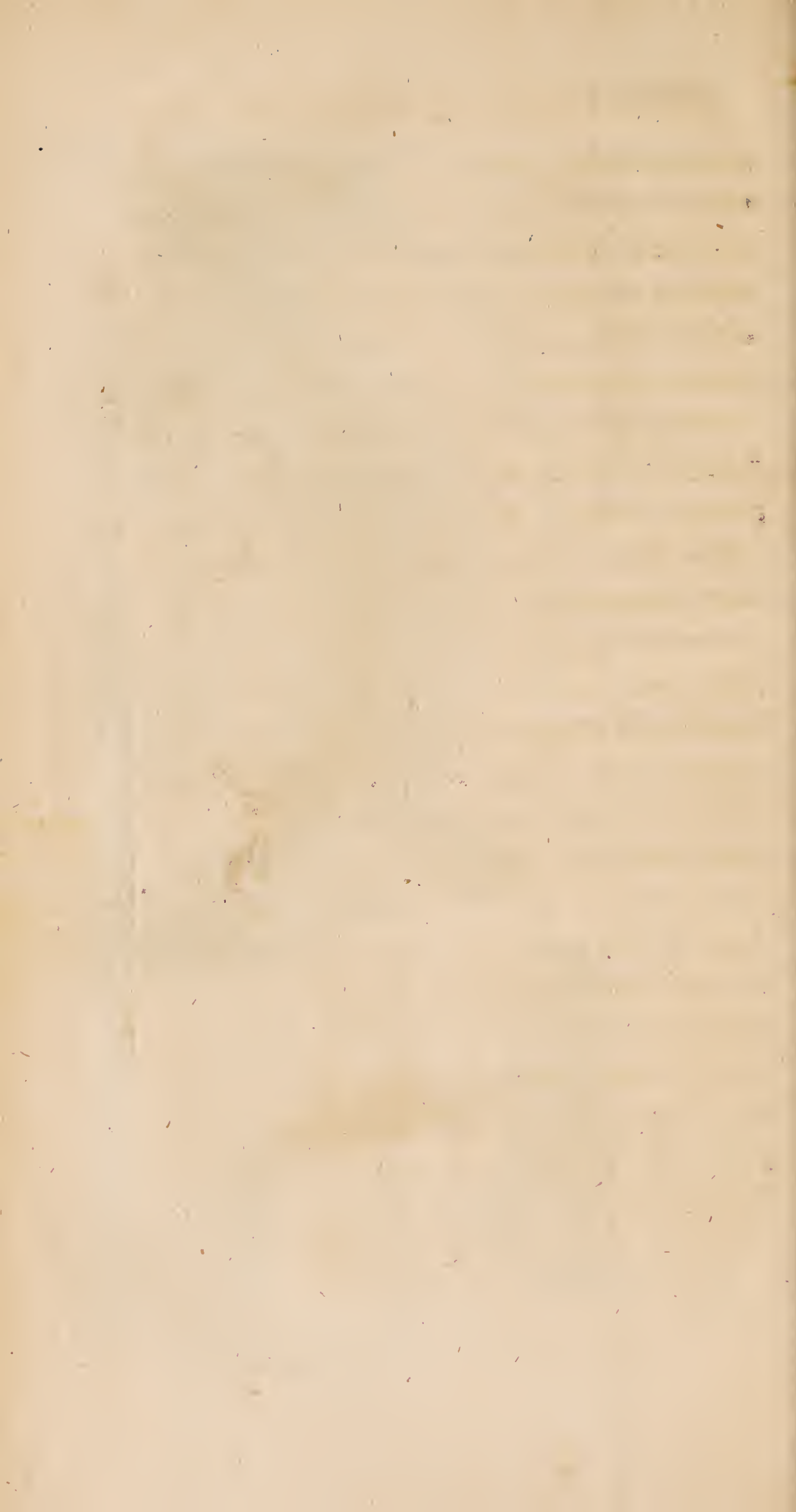


Fig. 5





other piece of intestine, when the common inflammation of the intestine often kills soon those it attacks ; of which I could give numerous examples, but shall relate only one. A Gentleman of weak nerves, and subject to *flatus* and pain in his stomach, was seized with a colic-pain about ten o'clock at night, for which he swallowed a small quantity of an ardent spirit. At three in the morning, twelve or fourteen ounces of blood were let from a vein in his arm, and a laxative clyster was injected, and operated well. At eleven that forenoon I first saw him, when his friends thought him much better, being free of pain ; but as his belly was greatly swelled and very tense, his pulse quick, small and intermitting, his eyes languid, his countenance faded, and a cold clammy sweat was over all his body, I made the *prognosis* of his having very few hours to live. He died before five of the afternoon, so that his disease killed him in eighteen hours ; and I have heard of others who died in less than twelve hours after the first appearance of inflammation.

THE

THE common practice of taking spirituous liquors, or the warm carminatives, when people feel colic-pains, is often unlucky, and public warning should be given against it; for tho' relief is found from such things in the windy or spasmodic colics, which is not a deadly disease, yet they hurry on the inflammatory ones so fast, that they soon prove mortal. I must likewise think, that writers on the inflammation of the intestines don't represent strongly enough the languor and low small pulse which such patients generally have more than in most other diseases. It is such, that I have seen several cases, where people of skill, deceived by these symptoms, have been afraid to order blood-letting, lest the patient had not strength to bear it, and thereby neglected this evacuation till it was too late. When there is a fixed pain in the stomach or intestines, with a quick tho' small pulse, no time is to be lost; blood ought immediately to be let plentifully, and venesection should be repeated till the pulse becomes full and free, which is a hopeful sign of a cure's being made, tho' neither pain nor fever have yet ceased.

THE *intusufceptio* or inflammation, but especially the latter, is generally the cause of what is commonly called the iliac passion or *miserere*; for the *volvulus* or twisting a part of the intestines into a knot, which was formerly said to be the case, is generally thought now, when anatomy is more cultivated, and inspection of morbid bodies is more universally allowed, to be an imaginary evil. It is very rare, but not impossible, as will appear from the history subjoined to this, and communicated to me by my Son.

ART.

ART. XXVIII.

A History of a genuine Volvulus of the Intestines; by ALEXANDER MONRO junior, M. D. and Professor of Anatomy.*

AN old man complained of a colic, which was neglected more than two days; when Mr. *William Wood* Surgeon in *Edinburgh* being called, found him in the agonies of death. Next day I obtained leave to open his *abdomen*, in presence of several students of physic. We saw fourteen inches long of the *intestinum ilium* hanging in a *sinus* down in the *pelvis*, all black and mortified, occasioned by a strangulation at the upper part of the two pieces of the *ilium* which formed the *sinus*. The firm stricture there was made by the *appendix vermiformis*, the body of which lay behind the constricted parts of the *ilium*, while the end of it passing over and before them, had sunk back again into a *plica* of the mesentery, from which, with great difficulty, I could draw it

* February 6. 1755.

it out, for it was there extended into a globular shape of three fourths of an inch diameter, by a glairy liquor, and was lodged in a depression of the mesentery, the entry to which was smaller than the cavity where the globular end of the *appendix* had been lodged.

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ART. XXIX.

*A Description of the American Yellow Fever,
in a Letter from Dr. JOHN LINING Physician at Charles-town in South Carolina,
to Dr. ROBERT WHYTT Professor of Medicine in the University of Edinburgh *.*

S I R,

CHARLESTOWN, December 14. 1753.

“ I N obedience to your desire, I have sent
“ you the history of the yellow fever as
“ it appeared here in the year 1748, which,
“ as far as I can remember, agreed in its
“ symptoms with the same disease, when it
“ visited this town in former years. In *this*
“ history, I have confined myself to a faithful
“ narration of facts, and have avoided any
“ physical inquiry into the causes of the fe-
“ veral symptoms in this disease; as that
“ would have required more leisure than I
“ am, at present, master of, and would per-
“ haps have been less useful than a plain de-
“ scription.

* March 7. 1754.

“ I wrote this history, so far as it relates
 “ to the symptoms and prognostics in the year
 “ 1748, when we had the disease last in
 “ this place ; intending afterwards, if it re-
 “ turned, to add, from further experience,
 “ the method of cure, and likewise an ac-
 “ count of any other symptoms which might
 “ attend it ; but, as no such opportunity
 “ has offered, I must now omit that part.
 “ However, I hope the description which I
 “ have given of this dreadful malady, which
 “ so frequently rages like the plague in the
 “ southern parts of *America*, is so full, that
 “ a physician may, from thence, not only
 “ form a true judgment of its nature, but
 “ likewise be able to deduce and communi-
 “ cate some more certain method of cure,
 “ than has perhaps hitherto been used.

“ I am sorry I could not give a fuller
 “ account of the dissections of those who
 “ died of this disease, having unfortunately
 “ lost my notes taken from those dissections.

I am, &c.

I. THAT

I. **T**HAT *fever*, which continues two or three days, and terminates without any critical discharge by sweat, urine, stool, &c. leaving the patient excessively weak, with a small pulse, easily depresso by very little motion, or by an erect posture; and *which* is soon succeeded with an icteritious colour in the white of the eyes and the skin, vomiting, hæmorrhages, &c. and these, without being accompanied with any degree of a febrile pulse and heat, is called in *America*, the *yellow fever*.

II. THIS fever does not seem to take its origin from any particular constitution of the weather, independent of *infectious miasmata*, as Dr. *Warren** has formerly well observed,

FOR within these twenty five years, it has only been four times epidemical in this town, namely, in the autumns of the years 1732, 39, 45 and 48, tho' none of these years (excepting that of 1739, whose summer and autumn were remarkably rainy) were either warmer or more rainy (and some of them
less

* In his Treatise concerning the malignant fever in *Barbadoes*, page 8.

less so) than the summers and autumns were in several others years, in which we had not one instance of any one being seized with this fever ; which is contrary to what would probably have happened, if particular constitutions of the weather were productive of it, without infectious *miasmata*. But that this is really an infectious disease, seems plain, not only from this, that almost all the nurses caught it and died of it; but likewise, as soon as it appeared in town, it soon invaded newcomers, those who never had the disease before, and country-people when they came to town, while those who remained in the country escaped it, as likewise did those who had formerly felt its *dire* effects, tho' they walked about the town, visited the sick in all the different *stadia* of the disease, and attended the funeral of those who died of it. And lastly, whenever the disease appeared here, it was easily traced to some person who had lately arrived from some of the *West-Indian* Islands, where it was epidemical. Altho' the infection was spread with great celerity thro' the town, yet if any from the country received it in town, and sickened on their return home, the infection

infection spread no further, not even so much as to one in the same house.

III. THE subjects which were susceptible of this fever, were both sexes of the white colour, especially strangers lately arrived from cold climates, *Indians, Mistees, Mulattoes* of all ages, excepting young children, and of those only such as had formerly escaped the infection. And indeed it is a great happiness that our constitutions undergo such alterations in the small-pox, measles and yellow fever, as for ever afterwards secure us from a second attack of those diseases. There is something very singular in the constitution of the Negroes, which renders them not liable to this fever; for tho' many of these were as much exposed as the nurses to the infection, yet I never knew one instance of this fever amongst them, tho' they are equally subject with the white people to the *bilious fever*.

IV. THIS fever began in the middle or rather towards the end of *August*, and continued till near the middle of *October*, when the weather became cold enough to prevent its further progress. In the beginning of *August*, the weather was warmer than I had
ever

ever known it in that month : the mercury in *Farenheit's* thermometer, for some days at 2 o'clock *p. m.* rose, in the shaded air, to the 96th degree, at which time several people died of apoplexies. The latter part of *August* and the first week in *September* were much more temperate ; the weather being then much as usual at that season of the year. The second week in *September* was cold, the wind being constantly easterly and the weather cloudy ; after which time I kept a register of the heat of the shaded air ; an abstract of which follows.

IN the latter part of *September*, and from the 1st to the 18th of *October*,

	<i>Septemb. Octob.</i>	
The mean heat at 2 <i>p. m.</i> was	72	65
The mean nocturnal heat was	68	54
The greatest heat at 2 <i>p. m.</i> was	79	75
The least heat at 2 <i>p. m.</i> was	60	52
The greatest nocturnal heat was	71	70
The least nocturnal heat was	62	42
The greatest increase of heat in 24 hours was	13	17
The greatest decrease of heat in 24 hours was	9	22
		IN

IN all the month of *September*, and in the greatest part of *October*, the wind was easterly.

THE depth of the rain in *August*, *September* and *October* respectively, was 6.881, 7.442 and 5.550 inches; which, tho' it exceeded the rain of these three months taken together at a medium, from the ten preceding years, by 5.570 inches, yet it was inferior to that which fell in the same months in several other years; for in the years 1747, 50, 51 and 52, there fell respectively, in those three months of these years, above 21, 22, 24 and 26 inches of rain.

V. FOR a day or two before the attack of the fever, people in general complained of a headach, pain in the loins and extremities, especially in the knees and calves of the legs, loss of appetite, debility and a spontaneous lassitude.

SOME however were seized suddenly, without any such previous symptoms.

VI. AFTER a chillness and horror, with which this disease generally invades, a fever succeeded, in which,

I. THE *pulse* was very frequent till near the termination of the fever, and was generally

rally full, hard, and consequently strong: in some, it was small and hard, in others, soft and small; but in all those cases, it frequently varied in its fullness and hardness. Towards the termination of the fever, the pulse became smaller, harder, and less frequent. In some there was a remarkable throbbing in the carotids and in the *hypochondria*; in the latter of which, it was sometimes so great, that it caused a constant tremulous motion of the *abdomen*.

2. THE *beat*, generally, did not exceed 102 degrees of *Farenheit's* thermometer; in some it was less, it varied frequently, and was commonly nearly equal in all parts, the heat about the *præcordia* being seldom more intense than in the extremities, when these were kept covered. In the first day of the disease, some had frequent returns of a sense of chillness, tho' there was not any abatement of their heat. In a few, there happened so great a remission of the heat for some hours, when at the same time the pulse was soft and less frequent and the skin moist, that one from these circumstances might reasonably have hoped that the fever would only prove a remittent or intermit-

tent. About the end of the second day, the heat began to abate.

3. THE *skin* was sometimes (tho' rarely) dry; but oftner, and indeed generally, it was moist and disposed to sweat.

ON the first day, the sweating was commonly profuse and general; on the second day, it was more moderate: but on both these, there happened frequent and short remissions of the sweatings; at which times the febrile heat increased, and the patient became more uneasy. On the third day, the disposition to sweat was so much abated, that the skin was generally dry; only the forehead and backs of the hands continued moist.

4. THE *respiration* was by no means frequent or difficult, but was soon accelerated by motion, or the fatigue of drinking a cup of any liquid.

5. THE *tongue* was moist, rough and white, even to its tip and edges. On the second day, its middle in some was brown. On the third day, the whiteness and roughness of the tongue began to abate.

6. THE *thirst* in very few was great.

7. A.

7. A *nausea*, *vomiting* or frequent *reachings* to vomit, especially after the exhibition of either medicines or food, came on generally the third day, as the fever began to lessen; or rather as the fulness of the pulse, heat, and disposition to sweat began to abate. Some indeed, but very few, on the first day, had a vomiting either bilious or phlegmatic.

8. VERY few complained of *anxiety* or oppression about the *præcordia* or *hypochondria*, nor was there any tension or hardness about the latter.

9. ON the first day they generally *dozed* much, but afterwards were very *watchful*.

10. *Restlessness* and almost continual *jactions* came on the second day.

11. A great *despondency* attended the sick from the first attack.

12. THE *strength* was greatly *prostrated* from the first attack.

13. THE *pain* in the *head*, *loins*, &c. of which they had complained (V) before the attack, were greatly increased, and in some, the pain in the forehead was very acute and darting; but those pains went generally off the second day.

14. THE

14. THE *face* was flushed, and the eyes were hot, inflamed and unable to bear much light.

15. ON the first day, many of them, at times, were a little *delirious*, but afterwards not until the recess of the fever.

16. THE *blood* saved at venæsection had not any inflammatory crust; in warm weather, it was florid like arterial blood, and continued in one soft homogeneous-like mass, without any separation of the *serum* after it was cold. When there was any separation, the *crassamentum* was of too lax a texture.

17. THE *stools*, after the first day, were fetid, inclined to a black colour, and were very rarely bilious, soft or liquid, excepting when forced by art; for an obstinate costiveness attended the febrile state.

18. THE *urine* was discharged in a large quantity, was pale, sometimes limpid, and rarely of a higher than a straw colour, except when the weather was very warm, and then it was more saturated, of a deep colour, and discharged in smaller quantities. It had a large cloud, except when it was very pale or limpid; but more generally it had a copious, white.

white sediment, even in the first day of the fever.

ON the second day, the urine continued to be discharged very copiously ; in some, it was then turbid, and deposited a more copious sediment, than on the first day ; this sediment was sometimes of a brownish colour ; in which case it was generally followed by bloody urine, either about the end of the second or beginning of the third day. The colour and quantity of the urine, discharged in equal times, were remarkably variable, being now limpid, then of a deeper colour, now discharged in a larger, then in a smaller quantity, which could not be ascribed to any change made either in the quantity or quality of the drink, &c.

VII. THE fever accompanied with those (VI.) symptoms, terminated on the third day, or generally in less than 72 hours from the first attack, not by any assimilation, or coction and excretion of the morbid matter ; for if by the latter, there would have been some critical discharge by sweat, urine, stool, or otherways, none of which happened ; and if, by the former, nothing then would have remained but great debility. No ; this fever did

did not terminate in either of these salutary ways, excepting in some, who were happy enough to have the disease conquered in the beginning by proper evacuations, and by keeping up a plentiful sweat, till the total solution of the fever, by proper mild diaphoretics and diluents. But those who had not that good fortune, however tranquill things might appear at this period, (as great debility and a little yellowness in the white of the eyes, seem'd then to be the chief complaints, excepting when the vomiting continued), yet the face of affairs was soon changed; for this period was soon succeeded by the second *stadium*; a state, tho' without any fever, much more terrible than the first: the symptoms in which were the following.

VIII. I. THE *pulse*, immediately after the recess of the fever, was very little more frequent than in health, but hard and small. However, tho' it continued small, it became, soon afterwards, slower and very soft; and this softness of the pulse remained as long as the pulse could be felt. In many, in this stage of the disease, the pulse gradually subsided, until it became scarce perceptible; and this, notwithstanding all the means used to support

support and fill it ; and when this was the case, the icteritious-like suffusion, the vomiting, *delirium*, restlessness, &c. increased to a great degree. In some, the pulse, after being exceedingly small and scarce perceptible, recovered considerably its fullness ; but that favourable appearance was generally of but short continuance.

2. THE *heat* did not exceed the natural animal heat ; and when the pulse subsided, the skin became cold, and the face, breast and extremities acquired somewhat of a livid colour.

3. THE *skin* was dry when the weather was cold, but was moist and clammy when the weather was hot.

4. THE *respiration* was natural or rather slow.

5. THE *tongue* was moist and much cleaner than in the former (VI. 5.) stage, its tip and edges, as also the gums and lips, were of a more florid red colour than usual.

6. VERY few complained of *thirst*, tho' they had a great desire for cold liquors.

7. THE *vomiting* or *reaching* to vomit increased, and in some was so constant, that neither medicines nor aliment of any kind were retained. Some vomited blood ; others
only

only what was last exhibited, mixed with phlegm; and others again had what is called the *black vomit* *. The reaching to vomit continued a longer or shorter time, according to the state of the pulse; for as that became fuller, and the heat greater, the reaching to vomit abated, and *è contra*.

8. THE

* That which is called the *black vomit*, at first sight, appears to be black; but on a more careful examination, I observed, that this colour proceeded from a great quantity of small, flakey, black substances which floated in the liquor thrown up by vomit; but the colour of this liquor was much the same with that which the patient had last drank, and was by no means black. Those black flakey substances are the bile mixed with, or adhering to the *mucus* which lined the stomach. For, upon dissection of those who died of this disease, not only in this but former years, I always observed that the *mucus* of the stomach was abraded, and the bile in its *cystis* was black and sometimes very viscid. In a Lad who died of this disease in the beginning of the fourth day, and who was immediately opened, the bile was not only black, but had the consistence of thick *Venice-turpentine*, and was exceedingly tough. On the inside of the stomach, there were several carbuncles or gangrenous specks. And in all those I have dissected, who have died of this disease, I have not only always observed the same, but likewise that the blood was very fluid, and the vessels of the *viscera* much distended; from whence I have been very inclinable to think, when the disease was not conquered in its first *stadium*, that, about the time of the termination of the fever, there was a *metastasis* of the morbid matter to the *viscera*.

8. THE inquietude was very obstinate, and when they dozed, their slumbers were but short and unrefreshing. There were some who were drowsy ; but these always awaked, after the shortest slumbers, with a great dejection of spirits and strength.

9. THE *jaælations* or restlessness was surprising ; it was frequently scarce possible to keep the patients in bed, tho', at the same time, they did not complain of any anxiety or uneasiness ; but if asked how they did, the reply was, *Very well.*

10. THE *debility* was so great, that, if the patient was raised erect in the bed, or, in some, if the head was only raised from the pillow, while a cup of drink was given, the pulse sunk immediately, and became sometimes so small, that it could scarce be felt ; at this time, they became cold, as in a *horripilatio*, but without the anserine-like skin : their skin became clammy, the *delirium* increased, their lips and skin, especially about the neck, face and extremities, together with their nails, acquired a livid colour.

11. THE *delirium* returned and increased ; it was generally constant in those whose pulse was small and subsiding.

12. THE inflammation of the *tunica conjunctiva* or white of the eyes increased much, but without pain.

13. A *yellowness* in the white of the eyes, if it did not appear before in the febrile state, became now very observable, and that icteritious-like colour was soon diffused over the whole surface of the body, and was continually acquiring a deeper saffron-like colour. In some indeed no yellowness was observable, excepting in the white of the eyes, until a little before death, when it increased surprisingly quick, especially about the breast and neck.

14. THERE were many small *specks*, not raised above the skin, which appeared very thick in the breast and neck; but less so in the extremities, and were of a scarlet, purple or livid colour.

15. IN women the *menstrua* flowed, and sometimes excessively, tho' not at their regular periods.

16. THERE was such a putrid dissolution of the blood in this *stadium* of the disease, that, besides the vomiting of blood formerly mentioned, and the bloody urine soon to be taken notice of, there were *hæmorrhagies* from

from the nose, mouth, ears, eyes, and from the parts which were blistered with *cantharides*. Nay, in the year 1739 or 1745, there was one or two instances of an hæmorrhage from the skin, without any apparent puncture or loss of any part of the scarf-skin.

17. AN obstinate *costiveness* continued in some ; in others, the stools were frequent and loose ; in some, they were black, liquid, large and greatly fatiguing ; in others, when the stools were moderate, even tho' they were black, they gave great relief ; in others again, the stools nearly resembled tar in smoothness, tenacity, colour and consistence.

18. THE *urine* was discharged in a large quantity, in proportion to the drink retained by the patient : it was pale if the patient was not yellow ; but if yellow, then it was of a deep saffron-colour ; in either case, it had a sediment, or at least a large cloud, which remained at the bottom of the glass ; in some, it was very turbid, in others, it was bloody, and the quantity of blood discharged with the urine bore always some proportion to the state of the pulse ; when that became fuller, the quantity of blood in the urine was diminished :

diminished: when the pulse subsided, the bloody urine increased, and even returned after it had ceased some days, soon after the pulse became smaller.

THIS stage of the disease continued sometimes seven or eight days before the patient died.

IX. WHEN this *stadium* (VIII.) of the disease terminated in health, it was by a recess or abatement of the vomiting, hæmorrhagies, *delirium*, inquietude, jactations, and icteritious-like suffusion of the skin and white of the eyes; while, at the same time, the pulse became fuller, and the patient gained strength, which, after this disease, was very slowly.

BUT when it terminated in death, those (VIII.) symptoms not only continued, but sooner or later increased in violence, and were succeeded with the following, which may be termed the *third stadium* of the disease, which quickly ended in death.

X. THE pulse tho' soft became exceedingly small and unequal; the extremities grew cold, clammy and livid; the face and lips, in some, were flushed; in others, they were of a livid colour; the livid specks increased

so fast, that in some, the whole breast and neck appeared livid ; the heart palpitated strongly ; the heat about the *præcordia* increased much ; the respiration became difficult, with frequent sighing ; the patient now became anxious, and extremely restless ; the sweat flowed from the face, neck and breast ; blood flowed from the mouth, or nose or ears, and in some, from all those parts at once ; the deglutition became difficult ; the hiccoughs and *subfultus* of the tendons came on, and were frequent ; the patients trifled with their fingers, and picked the naps of the bed-cloaths ; they grew comatous, or were constantly delirious. In this terrible state, some continued eight, ten or twelve hours before they died, even after they had been so long speechless, and without any perceptible pulsation of the arteries in the wrists ; whereas, in all other acute diseases, after the pulse in the wrists ceases, death follows immediately. When the disease was *very* acute, violent convulsions seized the unhappy patient, and quickly brought this *stadium* to its fatal end. After death, the livid blotches increased fast, especially about the face, breast, and neck, and the putrefaction

faction began very early, or rather increased very quickly.

XI. THIS was the progress of this terrible disease thro' its several *stadia*. But in hot weather, and when the symptoms in the first stage were very violent, it passed thro' those stages, as Dr. *Warren* has likewise observed, with such precipitation, that there was but little opportunity of distinguishing its different *stadia* ; the whole tragedy having been finished in less than 48 hours.

XII. IT was remarkable, that, 1. The infection was increased by warm and lessened by cold weather. 2. The symptoms in the several *stadia* were more or less violent, according to the heat or coolness of the weather. In hot days, the symptoms were not only more violent, but in those who seemed, in moderate weather, to be on the recovery, or at least in no danger, the symptoms were all so greatly heightened, when the weather grew considerably warmer, as frequently to become fatal. In cool days, the symptoms were not only milder, but many, who were apparently in great danger in hot days, were saved from the very jaws of death by the weather becoming happily cooler. 3. The
disease

disease was generally more fatal to those who lay in small chambers not conveniently situated for the admission of fresh air, to those of an athletic and full habit, to strangers who were natives of a cold climate, to those who had the greatest dread of it, and to those, who, before the attack of the disease, had overheated themselves by exercise in the sun, or by excessive drinking of strong liquors ; either of which indeed seemed to render the body more susceptible of the infection. Lastly, the disease proved most certainly fatal to valetudinarians, or to those who had been weakened by any previous disease.

XIII. THE *prognostics* in the first *stadium* are these, 1. The more acute and constant the pains are in the head, loins, knees, &c. the more the eyes are inflamed ; the greater their inability is to bear light, and the more the face is flushed at the first attack, the fever and all the symptoms (VI.) in the first *stadium* will be the more violent. 2. The more intense the symptoms are in the first state, the sooner will the fever terminate. 3. The sooner the disease runs thro' the first *stadium*, the shorter will be the duration of the

the second, & *è contra* 4. The shorter the duration is of the first, the greater and more certain is the danger in the second state. For when the fever terminated before the beginning of the third day, death seemed inevitably to be the consequence, as there was then no possibility of supporting the pulse, and as all the bad symptoms were then hurried on with such precipitation, that the patient generally died before the end of the fifth day, excepting a considerable coolness of the weather happily interveened; but on the contrary, it was a favourable circumstance when the fever was protracted to the end of the third day, without any remarkable hardness or depression of the pulse. 5. A great depression of the pulse, about the termination of the fever, is bad, since, from that circumstance, the vomitings, incessant jactations, the coldness and lividness of the extremities, hæmorrhagies, *delirium*, &c. are ushered in with surprising celerity. 6. The more the strength is prostrated from the first attack, the greater is the danger. 7. A vomiting coming on early in the disease, and continuing or increasing, is bad, and generally presages the *black vomit*. 8. A
sediment

sediment in the urine in the first and second day of the disease is bad, and the more copious the sediment is, the greater is the danger.

XIV. THE prognostics in the second *stadium* are these: 1. An early yellowness in the white of the eyes is bad: when it is observable about the end of the second day, in the first *stadium*, the patient generally dies about the beginning of the fourth day from the first attack of the disease. But when the yellowness does not appear till the end of the third day, if the patient does not recover, the disease sometimes continues to the 9th or 10th day of the second *stadium* before the patient dies. When the yellowness of the skin and eyes increases fast and acquires soon a deep icteritious-like colour, the greatest danger is to be apprehended. 2. If the inflammation of the white of the eyes increases, it is bad. 3. The more inflamed and bloody-like the skin is where it has been blistered, the greater is the danger. 4. If the vomiting continues or increases, it is bad; but the *black vomiting* is generally mortal. 5. When the pulse varies frequently in its fullness, being sometimes small, then

fuller, it is bad. But there was less dependence to be had on the pulse in this than is common in other diseases; for in some patients, in the second stage of the disease, even within a few hours of their death, the pulse, with respect to its fullness, softness, equality and frequency, has continued like that of one in perfect health, altho', from the other symptoms, the death of the patient could be foretold with great certainty. 6. The more the strength of the patient is reduced in the first, the greater is the danger in the second *stadium*. 7. Great restlessness, inquietude, an early *delirium* and a continuation of it are very bad. 8. Livid blotches about the neck and breast, a lividness of the lips and nails, flushing of the face, or a livid colour thereof, are sure signs of the quick approach of death. 9. Frequent loose stools, which give not any relief, are bad, and the sooner they spontaneously happen, the greater is the danger: but those which are black, and continue so without any abatement of the symptoms, are generally mortal. 10. Bloody urine and all hæmorrhagies, excepting slight ones from the nose, are bad; and the more copious they

they are, the greater is the danger. But a flux of the *menfes*, tho' not at their regular period, if attended with an abatement of the fymptoms, is a favourable circumftance, otherwife it is bad. II. A fuppreffion of urine, efpecially in thofe, who, in the courfe of the difeafe, have had large difcharges that way, is a certain fign of the quick approach of death.

XV. As to the prognoftics in the third (X) *ftadium*, it is fufficient to fay

“ Nature, alas! was now furpriz'd,

“ And all her forces feiz'd,

“ Before ſhe was how to refift advis'd *.

* Dr. *Sprat*'s Account of the plague of Athens.

ART. XXX.

*Answer to an Objection against Inoculation ;
by EBENEZER GILCHRIST M. D. Physician
at Dumfries *.*

INOCULATION has been practised here about five and twenty years, twenty almost constantly. The success of it soon demonstrated the great advantage and necessity of it, especially in a place long remarkable for bad small-pox. A letter I wrote two or three years ago to a Gentleman, who asked my opinion and advice for his children, was a means of introducing it into a part of the country where it had never been tried. While many were disposed to come into it, there were not wanting some, as in all such cases, to oppose it ; tho' in a pretty large trial of it, at the time, it had succeeded to the joy and satisfaction of all who wished well to it. Amongst other objections, one was more positively insisted on ; which, indeed, were it as well founded as it is specious, would

* February 5. 1756.

would effectually put to silence the warmest advocates for inoculation, and overturn altogether the practice. A proper answer was made to it ; and perhaps I should have thought no more of it, had I not been informed sometime after, that, in a society of ingenious Gentlemen, who made matters of this kind the subjects of their debates and inquiry, the same objection was taken notice of, and seemed to make an impression. Pains were taken to undeceive them also.

THE objection, to give it in the words of my friend's Letter, is this : “ The small-
 “ pox, in the ordinary way, is designed by
 “ nature as a drain to clear the constitution
 “ of some gross humours, which, if not car-
 “ ried off in this way, would bring on other
 “ dangerous diseases ; and for most part end
 “ in death, before persons arrive at middle
 “ age. Now, say the Objectors, the sup-
 “ puration where the small-pox is inoculated,
 “ is so inconsiderable, that it cannot be sup-
 “ posed sufficient to clear the body of those
 “ humours which are the parent of other
 “ destructive distempers. Besides, say they,
 “ this theory is justified by facts and expe-
 “ rience. Upon inquiry, it is found, that
 “ in

“ in those places where inoculation has most
 “ prevailed, *particularly in and about Dum-*
 “ *fries*, there are as many that die in child-
 “ hood, and before they arrive at the age of
 “ twenty, as formerly, even including those
 “ who are cut off by the small-pox. If this
 “ is the case, then inoculation is to no pur-
 “ pose.” I shall not trouble you with what
 occurred to me in answer to this objection.
 I greatly suspect the soundness of the prin-
 ciple on which it is built, and have ventured
 to deny the truth of the fact. How trifling
 soever you may think the objection, yet, as
 there is great stress laid upon it, and by per-
 sons of rank, I thought it might not be im-
 proper to apprise you of it. Thus far my
 friend.

An objection so plausibly formed, and with
 such particular application, seems to affect,
 more than any thing I have seen advanced,
 at once inoculation itself, and indirectly all,
 who, from a serious persuasion of its useful-
 ness, have shewn themselves industrious to
 promote it. To encourage and recommend
 a practice more hurtful in its consequences,
 than the immediate good of it can be of ser-
 vice, which could not escape the observation
 of

of every one, supposes want of attention at least, or something worse. Yet not so much to obviate a reflexion of this kind, because I believe undesigned, but for a more important reason, have I thought it demanded a public animadversion.

IN order to satisfy myself fully and others, I have not trusted wholly to my own opinion; but conversed with all who have been long and principally concerned in inoculating, thro' an extensive country: and we can affirm that of the inoculated, few are dead. Two or three of a hundred are the utmost we can recollect: but supposing them more, it is far short of the number that in ordinary circumstances die before twenty. Nor are we mistaken, do we think, when we say, that they are uncommonly healthy; which the small proportion that are dead will readily suggest to every one. It is impossible to be very exact; but it is sufficiently evident to us, that the state of the inoculated is much the reverse of what is objected. If this is true every where, as here it certainly has been hitherto, we are led to a very material discovery; and that which was intended as an unanswerable objection, by giving oc-
 casion

caſion to a pretty careful inquiry, has accidentally furniſhed a new argument in favour of inoculation, and a further proof of the great benefit of it. Long uſe has ſhewn it to be immediately a real ſecurity againſt the prevailing malignity of a very mortal diſtemper; and the preſent inſtance affords a ſtrong preſumption that it is, in its conſequences, no leſs a preſervative from many diſeaſes incident to a period of life the moſt fatal to mankind.

As to the theory in the objection, it is more philoſophical perhaps to argue thus: The fever of the ſmall-pox, communicated in the infant-ſtate, not only deſtroys or expells the latent ſeeds of diſeaſes, before they are, by time and accidents, perfected and put into action, but cauſes ſuch an alteration of the humours as may make them leſs ſuſceptible of any morbid impreſſions: and the veſſels being ſo ſoon accuſtomed, before they become rigid, to certain motions and extenſions, the body is rendered ever after more paſſive to the impulſes of any ſubſequent diſtemper; which therefore will be attended with leſs danger. This is agreeable

able to experience ; for one who has suffered an acute illness, will bear sickness better than another who never had the like distemper, and be less overcome by it.

FROM such reasoning it will seem to follow, that the sooner inoculation can be performed with safety, the greater will be the advantage. I cannot assert, that to this is owing the more than ordinary healthfulness of the inoculated with us, and that so few of them are dead : but if nothing forbid, I always advise it, the child yet unweaned ; and with me it has always happily and pleasantly succeeded. Though many have been inoculated before the sixth month, I chuse to delay it till this time, or any convenient time after, unless the subject is big and strong. Then, besides the greater pliancy, or kindly yielding of the solids, the blood has not, from the use of animal food, acquired an inflammatory disposition, and the humours a vicious taint. The first passages are not, as at a more advanced age, disordered by worms or other foulnesses. A formal preparation, the juices being all sweet and sound, is very little necessary : and the continual dread and dan-

ger of the natural infection is early removed; a matter, in this case, both for parents and children, not least to be considered. Teething, I know, is made the great objection here; but from this I have met with so little difficulty, that I make no scruple, except in a few circumstances, which may be easily discovered and avoided.

ART.

ART. XXXI.

*A Proposal of a new Method of curing obstructed Menfes; by Dr. ARCHIBALD HAMILTON Physician in Edinburgh *.*

IN a conversation I had sometime ago with my friend Dr. *Hunter*, now Physician in *Beverley*, concerning the cure of some particular diseases, I remember he proposed a method of removing obstructions of the *menstrua* by a mechanical compression of the external iliacs. I thought, from the anatomical structure of the parts, the proposal pretty reasonable in some cases; and resolved to put it into practice, the first favourable case that occurred to me. About six months ago, I was sent for to visit a girl betwixt nineteen and twenty years of age, who had been obstructed for near seven months, occasioned by suddenly exposing herself to cold, during the time she was menstruating. From the first appearance of her *catamenia*, to the time of their stoppage, she had enjoyed

a

* May 1. 1755.

a very good state of health. She had consulted no regular practitioner ; but had taken a few things, without any relief, that some of her female acquaintance had desired her. Her complexion was a little pale and wan. Her appetite and digestion bad, with eructations, and sometimes swelling of her stomach. She had now and then sickish and squeamish fits, with inclination to vomit. Her pulse was slow and languid, with a great lassitude and inactivity of body, not having a desire to take her usual exercise. On inquiry, I found she never had any pulmonary disorder, nor at present any complaint or uneasiness of her breast. She had also no pain or swelling about the *pudenda*. It now wanted about twelve days of the usual time of the approach of her *menfes*. I desired her to receive the steam of warm water, every night at bed-time, upon the *pudenda*, in order to relax these parts, so that the blood might more easily flow that way. I ordered her also ten days after, an aloetic purgative, to clean the *primæ viæ*, that the blood might find less resistance in its course when determined to the *uterus*. Next day, after she had taken the purge, I went and saw

saw her, and found it had operated four times. About seven o'clock that evening, I applied a compress and bandage to the crural arteries, at the same place where they put the *tourniquet* in amputations of the thigh, but not so tight as to endanger a mortification of the inferior extremities. At the same time, I desired her to sit above the steam of warm water. I intended to have staid with her, to observe the gradual effects of the bandage; but unluckily was sent for in a hurry to see another patient. I left strict orders, with a woman who was with her, to untie the bandage, in case she complained of any difficulty of breathing. On my return, about twenty minutes after, I found her in the same situation I left her in. Her pulse indeed beat about six strokes in the minute faster than before the application of the bandage. At the expiration of half an hour, she began to feel a sense of weight and fullness in the uterine region, and turned sickish. As her head and breast continued pretty easy, I begged of her to allow the bandage to continue somewhat longer, and gave her a spoonful of a cordial-julep. An hour and a half after the first application of the bandage, we found

found a visible appearance of the return of her *menfes*, by applying a piece of soft clean linen to the parts, which, when removed, was stained in several places. I slacked the bandage, as her legs were somewhat benumb'd, but was unwilling to remove it altogether till the discharge should continue to flow for sometime. I put her to bed; and, on my return next morning, found her still menstruating and easy. I now removed the rollers. The *menfes* continued to flow for three days, and returned regularly next period. Since that time, I understand she has been very healthy.

ART.

ART. XXXII.

*A Dropsy unexpectedly cured; by THOMAS LIVINGSTON Physician at Aberdeen *.*

AS those who are much conversant in the practice of physic and surgery, may be frequently disappointed in their favourable prognostics, so they may sometimes be agreeably surpris'd with the unexpected recovery of patients, whose symptoms they had pronounced mortal; an instance of which, I presume, will plainly appear in the history of the following case.

ROBERT DUNCAN, aged about 20 years, a labouring servant in the country, was brought to the Infirmary of *Aberdeen* June 19. 1753. He was so weak, that he could give no account of the origin or progress of his disease; but those who attended him from the country informed me, that during the preceeding winter he had been much exposed to cold, after which his belly and lower extremities began to swell; he contracted an intense thirst, with a paucity of urine; lost his

* February 6. 1755.

his strength and appetite; and the above symptoms continued to increase, without any means being tried for his recovery. He was admitted into the Infirmary, not with any prospect of relieving his complaints; but as he was destitute, and wanted the common necessaries of life, he was ordered a place on purpose to let him die in peace. He had a very large *ascites*, with an universal *anasarca*, particularly of his thighs and legs, which seemed ready to burst, and gangrenous-like vesicles appeared upon several parts of his legs and feet. His *scrotum* was distended to such an enormous size, that I could hardly at first distinguish the *penis*; and the above symptoms were attended with a *dyspnœa* and such a defect of *vis vitæ*, that I could not discover a pulsation in any of the arteries, and was only sensible of a tremulous sort of motion about his heart. In this dismal situation I gave him over to the care of a nurse, ordered him a little warm wine and water, and expected to hear of his death next morning; but I was disappointed.

June 20. His countenance appeared rather more lively, [he spoke a little with great difficulty, and I could discover a very languid pulsation

pulsation at his wrists; I sniped the vesicles on his feet and legs, and made several small punctures with a common lancet, into the most depending parts of the *scrotum*; there was a considerable discharge of a bloody-coloured *serum* from the vesicles; but the discharge from the punctures in the *scrotum* was clear and pellucid. I ordered warm stupes wrung out of a hot aromatic decoction, to be alternately applied to his *scrotum* and legs, and he got a large spoonful of the *julap. diuretic. Pharmacop. Paup.* every two hours, with wine and water for his ordinary drink.

———21. THERE was a considerable quantity of water discharged by the punctures in *scroto*, the size of which was sensibly lessened; but there came nothing from the vesicles on his legs and feet. I made several small punctures into the calfs of his legs, and continued the *fotus* and julep; his other symptoms much as yesterday.

———22. THERE was an incredible quantity of water evacuated from the punctures in his legs and *scrotum*; he had more strength, spoke more distinctly, and his breathing easier. Ordered his medicines to be continued as above.

—23. THE discharge from the punctures continued as yesterday, the swelling of his trunk and extremities was considerably diminished, his breathing easier, and pulse more distinct: he had some appetite: the same medicines were continued.

FROM the 23d to the 26th, the discharge continued; but turned daily less, and on the 27th was intirely stopped; his head, *thorax*, arms, *scrotum*, thighs and legs, quite free of swelling; but his belly still considerably distended, tho' not near so large as when he was admitted. He was reduced to the lowest ebb of weakness, and had a light nourishing restorative diet ordered him.

FROM *June 27.* to *July 4.* there was no very sensible change on the size of his belly; he used the *cerevisia diuretica* of the *Pharmacop. Paup.* was daily growing stronger, and took small doses of rhubarb and *pulv. scillitic.* at such intervals as his stomach could bear them.

FROM *July 4.* to *July 20.* he continued the use of the *cerevisia diuretica*, and took three of the *pilul. mercur. laxant.* every other night; these gave him two or three loose watery stools in the morning, which he bore very well;

well ; and from the time he began to use them, he passed his urine in much greater quantity than formerly.

July 25. HE was dismissed the Infirmary quite cured, and continued strong and healthy, on the 22d of *October* 1754, when this was wrote.

ART.

ART. XXXIII.

*History of a Patient affected with Periodic Nephritic Convulsions; by CORNWELL, TATHWELL M. D. Physician at Stamford *.*

A Fair healthy looking girl, of a full habit of body, aged 17, about the beginning of last *July*, without any warning, fell into violent and general convulsions, and a strong delirium, which lasted about six hours, then ended in sleep, and left a giddiness behind them for some time. As soon as the fit was off, she was blooded and blistered by her Apothecary, and seemed to have got well. In about three weeks after, a *diarrhæa* came on, and she complained a little of a slight pain in her back and bowels; and in a month from the first seizure, being a few days before the full moon, she had a second fit about as long as the former, which ended in the same manner. Soon after this fit, her tongue was of a dusky colour,

* May 1. 1755.

colour; her pulse quick, and her urine pale and small in quantity. The bleeding was repeated, a foetid plaister applied to the *abdomen*, and a few stools procured by pills of soap and aloes; after which there appeared a white sediment in the urine.

HALF an ounce of valerian was got down in saline draughts every day; in a week, a seton was put into her neck, and the week after she began to go into the cold bath every morning. Some bark and bitters, with a light chalybeate water, were added to the valerian, and the opening pills were used occasionally.

ABOUT the latter end of *August*, after several stools, a giddiness and sleepiness came on with a slight delirium, but without any convulsions.

WHEN she was waked out of this sleep, she complained greatly of a pain in her stomach and back; which neither V. S. the *semicupium*, nor *tinctura thebaica*, would relieve for any long time. The next day, on repeating the *semicupium*, she vomited, and then first complained of a *dysury*: I now gave her some pills of soap and *sal. martis*. On the evening after, the pain returning in
her

her back and *os pubis*, she was again put into the *semicupium*, and afterwards voided a considerable quantity of pale urine, which at first seemed to contain some rags, but soon hung with a white cloud, and let fall a whitish powder. The pain going off next day, the urine was more saturated, and as the *semicupium* soon made her faint and sick, it was omitted.

ALL this while the *catamenia*, which had first appeared about two years before, were entirely regular, and generally came on at their usual period about a week after each fit. She now continued the use of the pills, with *German Spaw-water* and the cold bath. The *seton* was dried up about the end of *September*, and she had only a slight return of pain in *November*, which was carried off by fomenting the *abdomen*. I recommended lime-water with her pills; but she has continued perfectly well ever since, without taking any thing.

I. WERE not these a kind of periodical *nephritic convulsions*? I have known nephritic symptoms particularly troublesome to other female patients about the time of menstruation;

menstruation; which seems to be owing to the greater turgescence of the neighbouring parts of the *uterus* at that period, by which the urinary vessels become at the same time fuller and more straitened.

2. IT is remarkable these convulsions were scarce accompanied with any symptom of the nephritic kind till the pain and *dysury* plainly appeared so, after the third fit, which the intermediate means seem to have contributed to make much flightier than the two former.

THIS will lead us, especially in nervous cases, which are often found to be sympathetic, to be more particularly attentive to, even, the smaller symptoms, which are sometimes a better clue to guide us to the origin of the disorder, than those violent accidents that are more apt to engross our attention.

8. THE *diarrhœa* preceeding the fits, was probably only an effect of the irritation of the urinary passages by consent of parts: since the keeping of her open afterwards seemed of service, instead of bringing on a relapse. Whence I shall only beg leave to observe, how necessary it is in practice to distinguish

distinguish symptoms that only precede others from those that produce others: in short, of what consequence it is not to mistake that for a cause which is only prior in point of time; especially as this, I fear, is too common a case, and often requires our utmost care, in observing the *juvantia* and *lædientia*, to avoid it.

ART.

ART. XXXIV.

History of a Fever after Child-bearing; by the same.*

A Delicate young woman (who, for two years before she was married, had, from a fright, been subject, at times, to a kind of hysteric epilepsy, especially at the approach of the *catamenia*) about four days after being safely delivered of her first child, had a milk-fever, which came on attended with languor, catchings and restlessness. She was carefully treated with mild diaphoretic and antispasmodic remedies both internally and externally. A miliary eruption came out upon her arms; the *lochia* flowed regularly, and the belly was kept moderately open: yet she got no rest, was delirious, her tongue dry and black, and her pulse quick and small. A blister was applied over her head, which, without any pain or strangury, produced a very large discharge. The next day, being the ninth from the beginning of

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the

* May 1. 1755.

the fever, she got some sleep, and became more sensible. A gentle perspiration was promoted by *sp. mindereri* in diluents, and hot bricks applied occasionally to her feet. The pulse was more quiet and regular, the tongue moister, and a white cloud subsided in the urine. On the 12th day, these flattering symptoms vanished—the *delirium* rose higher than ever: she was perpetually comatous or convulsed; breathed with difficulty; often rubbed her hands together, and picked the bed-cloaths; and the pulse could scarce be felt. A stimulant clyster was not retained, vesicatories would not rise upon her, yet she was excoriated with involuntary urine. In this deplorable extremity, *sinapisms* raised a blister on her feet, and a large flux was promoted from them by an emollient cataplasm, to which *unguent. ad vesicator.* was added *pro re natâ*.

As no medicine could be got down by the mouth, *pulv. cort. Peruv. unc. fs. in decoct. capit. papav. alb. unc. vi.* was thrown up by way of clyster, and repeated twice the next day with six drams each. These three, containing in all two ounces of bark, were retained for a week; in which time the bad symptoms

symptoms went off by degrees, and she took freely of both whey and other nourishment. Her feet still continued running, and were dressed with *unguent. alb.* Three more of the clysters with *scrup. vi.* each were given in as many days. She now rested well, and her pulse was become regular, was much emaciated, and so weak, she could scarce be raised without bringing on some hysteric symptoms. By degrees she began to recover her strength: the clysters were repeated now and then, increasing the quantity of bark in each to *unc. i.* Her appetite returned: in a fortnight she could be got out of bed: in another week, instead of the clysters, she took *extr. cort. Peruv. scrup. ii.* twice-a-day, occasionally interposing some *rhubarb*; in about ten days more she was got pretty well. The next month she had a slight fit of the kind she had been used to before marriage; but, by the help of some volatile fetid tincture in bitter wine, she got rid of these, and has continued free from them ever since, tho' she has had several more children.

ART. XXXV.

*History of a Fever with bad Symptoms; by the same *.*

A Middle-aged man, generally employed in a malt-kiln, about the latter end of *July* 1749, sometime after having fatigued himself, and got into a quarrel in a crowd at a public diversion, was seized with a fever; in the beginning of which, he was blooded, then had a vomit, and afterwards a great variety of diaphoretic medicines were tried with him. He was kept sweating, and several crops of eruptions succeeded each other, without any relief. Blisters were applied in several places, yet still a *delirium* and *vigiliæ* continued. Musk, and even the bark in substance, was given him; but all without any effect for above a month. The Apothecary who attended him in the mean time often urged the necessity of further advice; but his friends would not consent to it, till, at last, I was called in.

I

* May 1. 1755.

I found him in continual tremors and *subfultus tendinum*; his pulse was small and obscure, his tongue foul and dusky; and his urine had only a slight cloud in it. His eyes were so dim, he could scarce distinguish the persons about him; and it was very difficult to make him hear, especially on the right side. He was restless, languid, and delirious; often groaned and sighed, and was still bathed in a perpetual sweat. Some red spots appeared upon his breast, and many purple and livid ones on his back.

As he had been costive for some days, first a stool or two was procured by a warm clyster, some antimonial drops (which I have often experienced to deserve very justly the encomiums Dr. *Huxham* * has bestowed upon them as an excellent deobstruent) were given now and then in red wine. Elixir of vitriol and volatile tincture of bark were administered, first in saline draughts, and afterwards in tincture of roses. Within three days, his sweats were gradually checked, the red *papulæ* came to a kindly suppuration, the livid spots disappeared, the tremors, *subfultus*, and *delirium* went off. An abscess (which I had foretold

* Obs. de aëre, vol. i. p. 141.

foretold from the particular deafness on that side) broke in his right ear, and, together with a blister behind it, was kept constantly running. In a day or two more, all the bad symptoms were vanished; his pulse grew fuller and stronger, a good sediment appeared in his urine, and his tongue regained its natural colour. The volatile tincture was soon changed for a much larger proportion of the simple tincture of bark. In a week, not only his senses returned, but an appetite; and he began to recover his strength. A strong decoction of bark was added to the last mentioned tincture, by which, with a few doses of rhubarb interposed occasionally, the cure was compleated.

ART.

ART. XXXVI.

Accounts of extraordinary Motions of the Waters in several Places of North Britain, and of a Shock of an Earthquake felt at Dunbarton.

ONE great design of the institution of this Society being faithfully to record every remarkable *phænomenon* in nature that occurs, it has been thought proper to insert the following accounts of the effects of the late earthquakes, as they have been observed in our district of *North Britain*; nor do we regret, that they are not more singular or interesting in their kind. When all the facts and circumstances shall be collected by the united labours of the learned in different places, there is reason to expect, they may furnish materials for a more compleat and accurate history, than hath been transmitted of any like event that ever happened in any preceeding age of the world.

THAT a tremor, which is hardly to be felt at land, or which may altogether escape notice

notice there, may be very perceptible on the waters, will easily be believed ; nor is it more incredible, that a small concussion given to a great body of water will produce a very remarkable agitation in the narrow creeks and shallows. And it is observable, that these commotions were most violent in the deepest lakes, particularly in *Loch-ness*; the extraordinary depth of which hath been sometimes assigned as a reason for its never freezing, the severest winters not being able to reduce it to the coldness of ice.

I. *Letter from Mr. ROBERT GARDINER Commissary to the Army in North Britain to Dr. JOHN STEVENSON Physician in Edinburgh, giving an Account of the Agitation of the Waters of Loch-ness on the 1st of November 1755, when the City of Lisbon was destroyed by an Earthquake.*

SIR,

EDINBURGH, December 22d 1755.

“ I have your favour of the 20th, and, in
 “ compliance with your request, I give
 “ you the following reply to your queries.—
 “ I arrived at *Fort-Augustus* from *Fort-*
 “ *William*

“ *William* on the 31st of *October* last. Next
 “ morning about ten, I walked abroad, when
 “ the Barrack-Master and several others came
 “ and acquainted me, that they had seen a
 “ very extraordinary agitation in the waters
 “ of *Loch-ness*. I refused giving credit to their
 “ story, and a little afterwards returned to
 “ the *Fort*.

“ ABOUT eleven, my Clerk and the Brewer
 “ at that place came and acquainted me, that
 “ a more extraordinary agitation than the
 “ former had happened, and they apprehend-
 “ ed some danger to our brewery, which is
 “ situated where the river *Oich* discharges it-
 “ self into the lake. I walked then to that
 “ place ; but before my arrival, the water had
 “ returned to its usual channel. I saw very
 “ clearly the marks on the banks, to which
 “ it had flowed. The banks were quite wet,
 “ and a strip of leaves of trees and twigs,
 “ &c. left on them. I inquired then into
 “ this affair, and the account the specta-
 “ tors gave, were ; That they observed
 “ the river *Oich*, which runs from west
 “ into the head of the lake, swell very
 “ much, and flow up the river from the
 Vol. II. H h h “ lake

“ lake with a pretty high wave, about 2 or
 “ 3 feet higher than the ordinary surface,
 “ with a pretty quick motion against the
 “ wind, and a rapid stream about 200 yards
 “ up the river ; then broke on a shallow,
 “ and flowed about 3 or 4 feet upon the
 “ banks on the north-side of the river, and
 “ returned again gently to the lake : That
 “ it continued ebbing and flowing in that
 “ manner, for about an hour, without any
 “ waves so remarkable as the first, till about
 “ eleven o’clock, when a wave higher than
 “ any of the rest came up the river ; and,
 “ to the great surprize of all the spectators,
 “ broke with so much force on the low
 “ ground on the north side of the river, as
 “ to run upon the grass 30 feet from the ri-
 “ ver’s bank.

“ LIEUTENANT *Smith* of the artillery,
 “ Mr. *Gwyn* son of Captain *Gwyn* of the *Lock-*
 “ *ness*-galley, Mr. *Lumsden* Barrack-master
 “ at *Fort-Augustus*, Mr. *Forbes* Barrack-ma-
 “ ster at *Bernera*, *Thomas Robertson* Brewer
 “ at *Fort-Augustus*, and *George Bayne* my
 “ Clerk, and several others, were the specta-
 “ tors of this extraordinary *phænomenon* ;
 “ some

“ some of them saw the whole progress of it,
 “ others only a part.
 “ *Loch-nefs* is about 20 miles in length, and
 “ from 1 to $1\frac{1}{2}$ miles broad, and bears from
 “ south-west to north-east *. There was
 “ no extraordinary muddiness in the water,
 “ tho’ it did not appear quite so clear as
 “ usual. The morning was cold and gloomy,
 “ and a pretty brisk gale blowed from
 “ west south-west. The river *Oich* lies on
 “ the north side of the *Fort*, and on the
 “ south side, runs the river *Tarff* from west
 “ south-west, and discharges itself into the
 “ head of the lake, which was observed to
 “ be quite agitated at the same time and
 “ manner as the other. But there was no
 “ shaking or *tremor* felt upon the land.

2. Letter from Lieutenant ISAAC BARRE to
 DAVID ROSS, Esq; Sheriff-Depute of the
 Shire of Banff, concerning the Motion
 of the Waters of *Loch-nefs*, November 1.
 1755.

SIR, FORT-AUGUSTUS, January 15. 1756.
 “ I Have been here eight days, and, by
 “ the strictest inquiries, have at last
 “ picked

* It lies between the 57th and 58th deg. of northern latitude.

“ picked up the following account. Per-
 “ haps the whole may be of little conse-
 “ quence ; or, if it is, may come too late to
 “ be of any use to the Gentlemen who de-
 “ fired it.

“ ABOUT half an hour after ten in the
 “ morning of the 1st of *November*, *Loch-*
 “ *ness* was observed to rise in a most extra-
 “ ordinary manner at *Fort-Augustus* ; it rush-
 “ ed with great rapidity above two hundred
 “ yards up the river *Oich*, which runs into
 “ the Loch near the *Fort* ; it lasted by the
 “ best accounts about three minutes : the
 “ people of the country, unaccustomed to
 “ such an appearance, imagined at first it
 “ was caused by a great number of felks
 “ rushing up the Loch. There was no
 “ shock of any sort felt on shore, and the
 “ air was remarkably clear, and a very mild
 “ day, the wind then being westerly (and
 “ of course against the stream.)

“ I could get no account of any person’s
 “ being in a boat on the Loch at the time ;
 “ but what follows has an air of truth, be-
 “ cause the relater is reckoned an honest
 “ man and of great veracity ; and besides,
 “ had told the same circumstances frequent-

“ ly

“ly before he could suspect that this extra-
 “ordinary *phænomenon* would be the subject
 “of a serious inquiry.

“He went with a boat for wood, from
 “*Glen-morrison* to the *Old General's Hut* on the
 “opposite side ; and, about the time above-
 “mentioned, his boat being near loaded, as
 “he was coming to it from the hill, he
 “observed a sudden and violent wave co-
 “ming, which drove her several yards on
 “the beach, and returning, drew her back
 “into the Loch ; a second came with rather
 “more violence, and pretty near the same
 “effect ; a third came with a much more
 “extraordinary rapidity than either of the
 “former, and with more surprising effects ;
 “it drove the boat further on the beach,
 “took the wood intirely out of her, broke
 “off the rudder, and left her ashore full of
 “water. At this time there was a gentle
 breeze from the west, and a mild serene sky.

“THIS account was given by *James Fer-*
 “*guson* a Miller at *Folt-Soy* near *Glen-Morri-*
 “*son*, and also attested by his companion
 “*James Macdonald*.

“THEY assert, that the water, in their o-
 “pinion, upon examining the ground after-
 “wards,

“wards, must have risen at least three feet
 “perpendicular; and the time of the whole
 “agitation was about three minutes.

3. *Letter from Mr. JOHN ROBERTSON to
 JAMES SMOLLET of Bonhill, Esq; one of
 the Commissaries of Edinburgh, concerning
 the Agitation of the waters of Loch-lomond,
 November 1. 1755*.*

SIR, RosDOE, December 17. 1755.

“AS I have been from home for a fort-
 “night past, your’s came to hand only
 “yesterday, or would have answered it
 “sooner. The task you impose upon me,
 “viz. the giving an account of the commo-
 “tion that was observed in the waters of
 “Loch-lomond on the 1st of November, is al-
 “ready executed by Mr. Macfarlane the Mi-
 “nister of Arrochar, according to the best
 “accounts he could get of it from the spe-
 “ctators; as I was not an eye-witness of
 “this uncommon *phænomenon*, and as the ac-
 “count he gives of it is perfectly agreeable
 “to the relations of all those who saw it,
 “I

* *Lochlomond* lies upwards of fifty *English* miles west of
Edinburgh, about the 56th degree of north latitude.

“ I will write you an exact copy of his re-
 “ lation, which, I think, is as full and dis-
 “ tinct an account as can be got of it.

“ ON the 1st day of *November* last, *Loch-*
 “ *lomond*, all of a sudden, and without any
 “ gust of wind, rose against its banks with
 “ great rapidity, and immediately retired ;
 “ and in five minutes time subsided, till it
 “ was as low in appearance as any body then
 “ present had ever seen it in time of the
 “ greatest summer drought, and then it in-
 “ stantly returned toward the shore, and in
 “ five minutes time rose as high as it was
 “ before ; and the agitation continued at
 “ this rate from half an hour past nine fore-
 “ noon till fifteen minutes past ten, taking
 “ five minutes to rise and as many to sub-
 “ side ; and from fifteen minutes after ten,
 “ till eleven, every rise came somewhat short
 “ in height of the immediately preceeding
 “ rise, taking five minutes to flow, and five
 “ to ebb, until the water settled as it was
 “ before the agitation. After the agitation
 “ was over, the height to which the water
 “ had rose was measured, and found to be
 “ 2 feet 4 inches perpendicular. *Loch-long*
 “ and *Loch-keatrin* were agitated on the same
 “ day

“ day, and about the same time ; but the
 “ *phænomenon* was not so minutely observed
 “ as that any exact account can be got of it.

“ THIS relation of Mr. *Macfarlane*’s of the
 “ *phænomenon*, contains in it every thing re-
 “ markable I have as yet heard concern-
 “ ing it.

4. *Letter from Mr. MARK McCALLUM to
 GEORGE CLERK, Esq; concerning an un-
 usual motion of the Waters of the Frith of
 Forth at Queens-ferry, seven miles west of
 Edinburgh, on the 1st of November 1755.*

S I R,

“ TO the best of my remembrance, on
 “ **T** Saturday the first of November, a-
 “ bout ten o’clock before noon, being then
 “ on the pier at *Queens-Ferry*, I observed the
 “ water to rise very suddenly, and return
 “ again with the same velocity about a
 “ foot or eighteen inches perpendicular, 2
 “ near as I can remember ; the barks and
 “ boats then afloat ran forwards and back-
 “ wards with great rapidity, and this conti-
 “ nued for the space of three or four mi-
 “ nutes ; but after the second or third rush
 “ of

“ of the water, it gradually abated. There
 “ was no wind at the time the waters were
 “ agitated as above.”

5. *An Account of an unusual Motion in the Waters of Closeburn-Loch in Dumfriesshire, on Sunday the 1st of February 1756, in a Letter to GEORGE CLERK, Esq; **

SIR, CLOSEBURN, February 4. 1756.

“ ABOUT a quarter before nine on Sunday
 “ morning, we were alarmed with an
 “ unusual motion in the waters of Closeburn-
 “ Loch. The first thing that appeared to
 “ me in this wonderful scene, was a strong
 “ convulsion and agitation of the waters from
 “ the west side towards the middle, where
 “ they tossed and wheeled in a terrible man-
 “ ner. From thence proceeded two large
 “ currents, or more properly rivers, which
 “ ran with a swiftness and rapidity beyond
 “ all description quite contrary ways; one
 “ from the middle to the south-east, and
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* It may not be amiss to observe, that, on the 1st of February 1756, according to advices from abroad, a considerable earthquake happened at Oporto in Portugal.

“ the other to the north-east points of the
 “ Loch. There they were stopped short,
 “ as the banks are pretty high, and ob-
 “ liged to turn ; which occasioned a pro-
 “ digious tumbling and agitation at both
 “ ends of this body of water. There was
 “ likeways a current which rose some-
 “ times considerably above the surface, near
 “ the west side, that I frequently obser-
 “ ved running with great velocity a hun-
 “ dred yards to the southward, and re-
 “ turning in a moment with as great ve-
 “ locity the other way. What I observed
 “ in the next place, was the tossing of
 “ the waters in the ponds, which were
 “ more or less moved as the agitation of
 “ the Loch came near this side, or kept
 “ at a greater distance from it. But, as
 “ it is beyond my capacity to give a particu-
 “ lar description of all that happened on this
 “ occasion, I shall conclude with telling you,
 “ that the agitations and currents above men-
 “ tioned continued, without intermission, for
 “ at least three hours and an half, or four
 “ hours, when they began to abate a little in
 “ their violence, tho’ the tossings and cur-
 “ rents were not quite over at sunset. I had
 “ almost

“ almost forgot to tell you, that this *phæno-*
 “ *menon* was renewed on *Monday* morning a
 “ little before nine, and lasted for an hour
 “ and an half; but the motion of the waters,
 “ &c. were not so violent as the day before.
 “ What is very remarkable, there was not
 “ the least gale of wind on *Sunday* till one
 “ o’clock, which helped us not a little in our
 “ observations.”

6. Letter from Mr. PANTON relating to the
Earthquake felt at Dunbarton.

S I R,

DUNBARTON, *January 17. 1756.*

“ I N answer to your’s relative to the earth-
 “ quake here, there happened but one
 “ shock, and that very moderate, before one
 “ of the clock *December 31st 1755*; it con-
 “ tinued for a very small space of time. It
 “ agitated some people in bed very percepti-
 “ bly, and was felt by Mrs. *Weir* and some
 “ others, who were not gone to bed. It
 “ had a sensible effect upon tame birds in
 “ cages, and fowls: it so alarmed both, as
 “ to make the former flutter prodigiously,
 “ and the latter to croak in a frightful man-
 “ ner: it shook the board out of one cage,
 “ and

“ and spilt the water in the glafs thereof. It
 “ was equally felt by thofe who lived in
 “ ground-ftoreys, as by thofe in fecond and
 “ third ftoreys. There were fome fconces
 “ in Mrs. Colquhoun’s houfe obferved to vi-
 “ brate during the fhock. Nothing more
 “ happened worth noticing.

N. B. This earthquake was felt at the fame
 time in *Glasgow*, *Greenock*, and other places
 of the neighbouring country.

End of the Second Volume.

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